

THE PSYCHOLOGICAL REVIEW.

ON RELATIONS OF TIME AND SPACE IN VISION.

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In the ordinary vision of daily life the eyes, the head and the whole body are in continual movement. There are no distinct and lasting images on the retina; the physical conditions are those of the photographic plate when the camera is constantly moved hither and thither. But the world that we see appears to each of us distinct and unshifting. When I glance across the room—along a row of books covering its side, for example—images follow one another on each retinal element in rapid succession, but I see this time continuum as a space continuum with all the objects duly arranged side by side.

When black and white surfaces are moved across the retina with the color-wheel at the rate of fifty white stimuli per second, a gray, as every one knows, is seen. When, however, the eye moves so that the line of sight passes over black and white surfaces at the same rate, there is no fusion whatever, the surfaces being seen distinctly side by side. No fusion occurs even when 1,000 stimuli per second fall upon each retinal element.

When colored surfaces are moved one after the other across a limited part of the retina, under conditions described below, they do not seem to follow each other on the same field, but the colors are seen spread out side by side or intermingled in a larger field than is actually presented. The arrangement in space of the stimuli varies greatly with different observers, each seeing them in a way peculiar to himself. Thus in the elabora-

tion of a time series into a spatial continuum the same stimuli are followed by perceptions different for each observer.

These phenomena are important from several points of view. They indicate that the fusion of stimuli—and indeed all the phenomena of color vision—are not the result of chemical processes in the retina, but are cerebral phenomena dependent on complex factors. The individual differences under unusual experimental conditions indicate that the processes of vision are not organically fixed, but are the result of individual experience and adjustment. The perception of a time series as a spatial continuum shows that our perceptions are not ‘copies’ of a physical world, but are mental phenomena dependent on utility and the whole content of present and past experience.

SECTION I. PERCEPTION WITH THE MOVING EYE.

That in ordinary vision the spatial world of perception is reconstructed from a series of changes in time of the retinal images is an observation which, so far as I am aware, has not hitherto been made. But when once stated the fact is obvious and scarcely needs confirmation by special experiments. The results, however, when stated in quantitative terms, show a remarkable sensitiveness in the visual mechanism.

When the eyes are moved horizontally through an angle of about 45° once a second, from a distance of about 5 m., sweeping over an arc of about 4 m., on which are black and

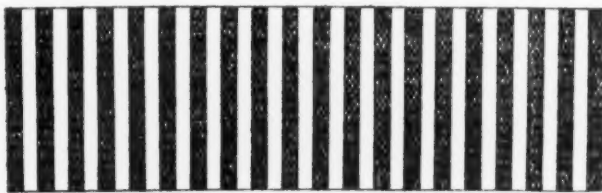


FIG. 1. Black and white bars which do not fuse when the line of sight is moved over them.

white lines 2 mm. wide, as shown in the figure, stimulations at the rate of one thousand per second fall on each retinal area. There is no fusion into a gray, or blurring of the lines, so long

as the eyes are kept in focus for the arc. The white lines stimulating the retina in rapid succession are seen clearly side by side exactly as when viewed with the stationary eye, except that the field of distinct vision is enlarged.

When the eyes move so that the line of sight passes over the field at the rate of about 2 m. per second, going from a white to a black surface, it will in $\frac{1}{50}$ sec. move over 4 cm. According to our experience with the color-wheel there is fusion when one stimulation of the retina follows another at an interval of $\frac{1}{50}$ sec., or less, and if the same conditions obtained with the moving eye the white edge would not be seen, but white and gray would extend 4 cm. and more into the black. But as a matter of fact the white edge is seen with perfect distinctness, there being no fusion whatever. If one looks at the ground when riding in a trolley-car all the objects are fused, but it is easy to move the eyes so quickly in the opposite direction that the objects are distinctly seen. If when the car stops the line of sight is moved over the ground at the same rate there is no fusion whatever.

The rate of stimulation by movements of the eye cannot be increased greatly beyond that given above, and the limits of distinct vision are not set by the inability of the retina to respond to the stimuli, but by the 'minimum visible' and the maximum rate of movement.¹ More than a thousand interruptions per second give a series of sharply defined retinal processes. Vibrations fuse into a continuous tone when there are thirty or less per second, but two noises can be distinguished when separated by an interval of about .002 sec. Touches fuse at about the same interval. It is always stated that hearing and touch respond more accurately to successive changes in stimuli than vision, but this is apparently not the case.

The head can be moved more quickly than the eyes, and the whole body can also be turned more quickly than the eyes. I am not aware of attention having been called to this fact, which is of some interest in view of the phenomena here described. When

¹Conflicting results as to the rate of movement of the eye have been obtained by Lamansky (*Pflüger's Archiv*, 2: 418-422, 1869), Erdmann and Dodge (*Ueber das Lesen*, Halle, 1898) and Huey (*Am. Journ. of Psych.*, 2: 283-302, 1900).

the head or body is turned quickly objects in the field of vision appear to move in a direction opposite to that of the line of sight, and this is the more marked the quicker the movement. When the eyes only are moved the field does not shift,¹ or if so the apparent motion is very slight. When the line of sight is moved by turning the head so that it passes over black and white bars at such a rate that there are about fifty stimuli per second there is no fusion. But when the movement is more rapid the field begins to move and there is then blurring and finally gray.

The fact that we see clearly and side by side objects over which the line of sight moves rapidly is evident. I have no doubt that the explanation I have given is correct—namely, that the visual mechanism is sufficiently sensitive to respond to more than 1,000 stimuli per second, and that we perceive a series of time changes on the retina as a spatial continuum because for our reactions it is in fact a spatial continuum. The only other possible explanations seem to be that the moving eye makes a series of jumps, seeing at each stop a distinct field, or that the distinctness of the field is an illusion, being really a memory image. But these suggestions are invalid. It can be readily observed that the eye does move continuously over the field. Further the field of distinct vision² is only about 1° , and to see all parts of an arc of 90° by separate steps the eye would need to stop some ninety times. It takes about $\frac{1}{5}$ sec. for the eye to stop and fix a field, and it is of course absurd to suppose that it could stop frequently in the half second needed to sweep over the arc. Finally if one had in fact only a few distinct images in the course of the movement it would remain true that a series of successive images is seen as a space continuum.³

¹ James (*Psychology*, 2: 173) is apparently mistaken in stating that it does, not discriminating movements of the eyes from movements of the head and trunk. The difference in the case of the moving eyes and the moving head may in part be due to the fact that with the eyes all objects in the field of vision maintain their relative positions, whereas when the head is moved nearer objects move over further ones, and we learn to regard a moving field as natural and a thing to be ignored.

² I do not find any measurements of the field of distinct vision, beyond the estimates of Weber and of Erdmann and Dodge. The fovea is said by Kölliker to be from 0.18 to 0.225 mm. in diameter, by Dimmer 1.1 mm. or more.

³ In reading a line of printed text the eyes do stop as they sweep over the page. This, however, is not because there is any blurring of the letters with

The second alternative, that the distinct vision with the moving eye is an illusion, is at least conceivable. In daily life one does not notice that at each moment only a very small part of the field of vision is distinct. But this phenomenon is in fact explained by the constant movements of the eyes and the clear vision accompanying the movements—not conversely. Further it can be shown experimentally that when the eye sweeps over an object it is seen distinctly even though the observer may not know what it is. In this case if fusion occurred, as it does when the field is in motion, it would not, *e. g.*, be possible to discriminate red and green bars from white and black bars. Finally the perception as a space continuum of a series of changes in the time of stimulation is confirmed by the experiments with a moving field described below.

SECTION 2. THE FUSION OF MOVING OBJECTS.

The fact that the sense of vision responds to changes in stimulation as accurately as the other senses is not in itself surprising. I have always thought¹ that the phenomena of binocular vision, of contrast, of after-images, etc., are far too complicated to be attributed chiefly to the retina, and have never cared for the physiological mythology of the Young-Helmholtz and Hering theories of color vision. Assuming it to be true as a matter of experience that the moving eye responds accurately to changes in time of stimulation, it does not seem to me that this fact particularly requires explanation. On the contrary we need to explain why lights fuse when in motion. In this connection it may be suggested that vibrations producing a simple tone fuse at 30 or fewer per second, which is at nearly the same rate that black and white fuse on the color-wheel. When a slowly vibrating tuning-fork is touched with the finger we also have a partly continuous sensation. The conditions with these different senses are thus somewhat analogous. We have under certain

the moving eyes, but owing to the limited amount that can be simultaneously perceived and the time it takes to perceive it.

¹I have said: "The writer finds most satisfaction in assuming that the continuity of physical vibration is transmitted through the retina to the brain, where inertia, summation and inhibition intervene to produce the changes which are correlated with consciousness." *PSYCHOL. REV.*, 1, 326, 1894.

conditions fusion with comparatively few changes per second, and under other conditions can discriminate stimuli much closer together. I maintain that we should attribute the phenomena of perception chiefly to utility. We perceive what it is useful for us to perceive in order to direct our actions so as to preserve ourselves and attain our ends, and we usually perceive things in the way that is most conducive to these purposes. A general principle such as this does not of course obviate the need of investigating the conditions in the different cases, but it may lead us to consider that it is circumstances in which our senses fail in utility that need explanation rather than cases in which they fail to give copies of an external world, which is itself only the chief case of the interpretation of perceptions in accordance with maximum utility.

We can discriminate two noises separated by $\frac{1}{500}$ sec., but vibrations fuse into tones varying in quality when separated by much longer intervals. We notice very small differences in pitch, and the qualitative differences are evidently more useful to us than would be a series of sounds at varying rates. Beats on the other hand, though at the same rate as the vibrations giving tones, do not fuse perfectly.

In the case of vision a great loss in efficiency would result if the organs concerned could not respond to changes in the image as rapidly as these are brought about by movements of the eye. There would be confusion just at the time when clear vision might be of special importance. The processes of vision and of movement have developed in harmony, and the sensitiveness of the retina and of the brain centers is as great as is needed. The comparative slowness with which the eyes can be moved is perhaps due to the fact that a greater rapidity would be of no use, as it could not be followed by the retina and brain.

In the case of moving objects on the contrary, it is not necessary and would probably be disadvantageous for us to see the separate phases. The savage is concerned with the fact that a panther is springing toward him or a deer running away, not with the successive positions of the limbs of the creatures as instantaneous photography would exhibit them. Moving objects

in nature are seen as wholes as they should be for our convenience. When we produce motion by machinery or in the laboratory with the color-wheel we also naturally have fusion, but I doubt whether we do produce complete fusion with the color-wheel. It is assumed that we do, but I have never seen it. There is on the color-wheel always an appearance of glitter or translucence similar to that in binocular fusion. I have never been able to make gray in a color-wheel from red and green (with the necessary correction of blue), but when it is as nearly gray as it can be got I see both red and green with an appearance of translucence. Neither do I see a gray when red and green are mixed by reflection or diffraction; there is in this case an oscillation comparable to the conflict of the fields in binocular rivalry. If I am correct in these observations it is evident that we do not have complete chemical fusion on the retina.

I may refer in this connection to another phenomenon of the color-wheel, which I observed some years ago, but have not described. When we mix black with white we get a blue-gray. This is well known, and is said to be a case of the Purkinje phenomenon, according to which lights are said to become bluer as they are made fainter.¹ But if we throw a shadow on the same white that we are mixing with the black (say a perfect black from a black hole) we can get a gray evidently lighter or darker than the gray from fusion, but this cannot be matched, the shadow being yellow-gray as contrasted with the blue-gray on the color-wheel. The intermittent stimulation of the color-wheel seems to produce the blue,² and Talbot's law can scarcely be exactly correct. This experiment also indicates that in the fusion of successive stimuli we have not a chemical process in the retina, at least not such a simple one as is assumed.

The phenomena of after-images are not at variance, as might perhaps be supposed, with the ability of the eye to respond to very rapid time changes in the environment. A perceptible

¹ As a matter of fact the Purkinje phenomenon does not occur when the intensity of the colors is altered, but only when the eye is accommodated for a fainter light. I accidentally discovered this fact in trying to demonstrate the phenomenon to my classes in a light room. It has been described by Hering (*Pflüger's Archiv*, 60: 519-542, 1895).

² The degree of blue is, however, independent of the rate of intermission.

after-image is only produced by a certain minimum quantity of light,¹ and with the moving eye in ordinary vision the time that any object is fixed is extremely short, while the intensity of the light is usually small.² It is exactly the delicacy of the visual mechanism which would lead us to expect an after-image when the light is unusually intense or fixed for an unusually long time. It is a general principle that a nervous system sufficiently delicate to respond to ordinary stimuli is overstimulated and perhaps injured by extraordinary stimulation. Thus after-images, like the expression of the emotions in certain cases, are not useful reactions but pathological conditions. I can at present see an after-image obtained about six years ago.³ This is not a purely retinal brand, because it can be made more distinct and, I think, permanent by acts of attention.⁴ After-images do not in any way contradict the sensitiveness of the visual mechanism to rapid time changes, but on the contrary the indications that they are cerebral phenomena, and the fact that being useless they are ignored in our ordinary vision, support the general point of view of this paper.

SECTION 3. THE PERCEPTION OF MOVING OBJECTS.

When the color-wheel is turned so slowly that fusion does not occur there is a flicker if the sectors differ in intensity, and

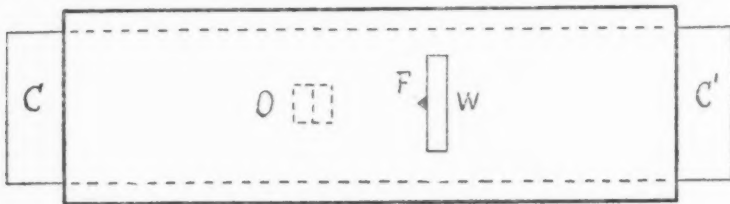


FIG. 2. Simple apparatus for studying the perception of moving objects.

¹ Cf. Franz, *THE PSYCHOLOGICAL REVIEW*, *Monograph Supplements*, No. 12, 1899.

² As a matter of fact the after-image appears only after a latent period, which contradicts the ordinary theory that it is simply the continuation of the commotion produced by the stimulus on the retina.

³ Cf. my article on 'The Perception of Light' in *System of Diseases of the Eye*, Philadelphia, 1897.

⁴ Newton made a similar observation. Cf. his letter to Locke in Brewster's *Memoirs of the Life of Sir I. Newton*, Edinburgh, 1855.

the colors intermingle. My research was originally concerned with experiments on moving objects and commingling colors, which led to the results on perception with the moving eye already described. Four methods were used to exhibit the stimuli, and it is desirable to describe these in some detail, as they may be used for various purposes.

(1) The phenomena may be produced by very simple means with sufficient accuracy for demonstration. All that is needed is a black screen about 30 cm. long and 10 cm. wide with a slit perpendicular to the long side 5 cm. long and

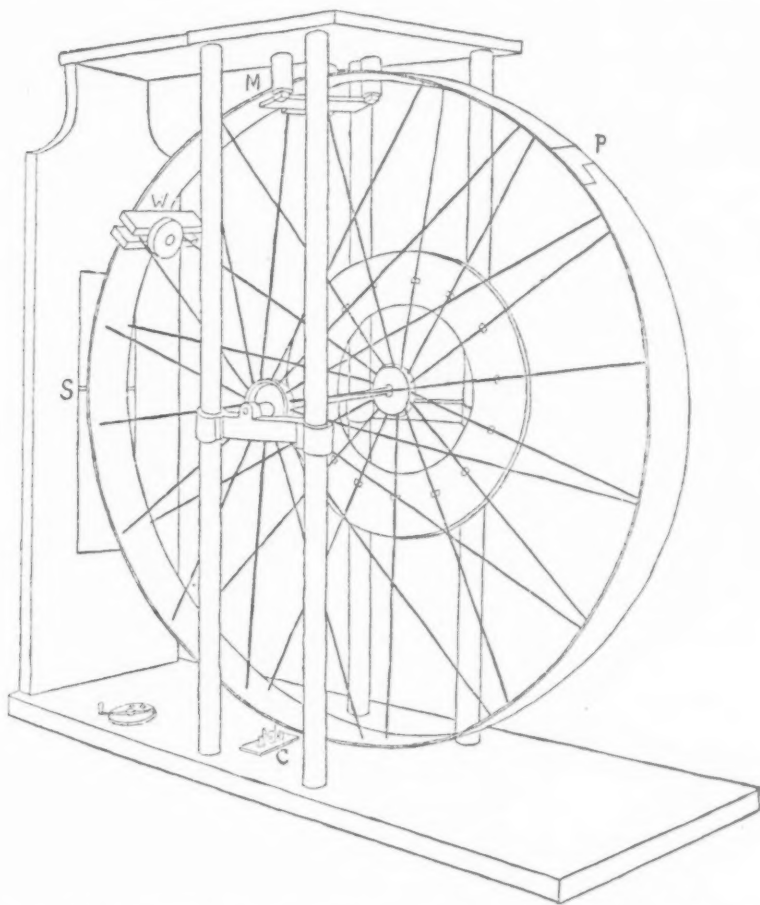


FIG. 3. Wheel chronoscope for studying the perception of moving objects. The rate of motion, size of field, etc., can be accurately adjusted.

1 cm. wide. The screen is so attached to a board that a long card (CC') can be slid beneath it. On the card are attached colors or shapes (O) which are seen as they pass the window (W). The card can be moved with the hand at rates varying up to about 5 m. per second with a mean variation of about one tenth.¹ Thus if green and red bars each one cm. wide be pasted on the card, green can be exhibited for about .01 sec. and the same retinal elements immediately afterwards stimulated by red for the same period.

(2) In order to secure greater accuracy and convenience in these experiments and for other psychological purposes I had constructed² an instrument which may be called a wheel chronoscope. It consists of a wheel, one meter in diameter with a rim about 9 cm. wide. Cards or colors placed on the outside of the rim pass by the slit (S) and may be seen by the observer on the far side of the instrument. It is thus possible to exhibit a color or a series of colors for an interval dependent on the rate at which the wheel moves and the size of the colored surfaces. The intensity of the light and the size of the field can also be varied, thus permitting experiments on the relations of time, area, intensity and color in vision. The instant at which the color reaches the slit can be recorded by keys at C , thus permitting experiments on the time of discrimination, etc. The wheel can be revolved at a constant rate by a motor or can be turned by the fall of the weight W . The latter method was used in the present experiments. The wheel is in this case held in position by the electro-magnet M . When the current is broken the weight falls and the wheel swings like a pendulum. At and near the dead point the rim is moving at a nearly uniform rate and at this point the colors pass the slit in the screen. The rate can be adjusted by the weight W , and was in these experiments 1 m. per second. The object can be readily attached to the rim at P out of sight of the observer and exhibited by breaking the current of the electro-magnet. On the return swing the wheel is caught up to the magnet, the slit being in the meanwhile covered. If a green surface 5 cm. wide is attached at F , followed by a red surface 5 cm. wide, the observer has over an area of the retina through the slit, say 1 cm. wide, first green exhibited for 50 σ ($\frac{1}{20}$ sec.) and then following it in the same area red for 50 σ .

(3) A chronograph³ was also used to exhibit the colors. The horizontal drum with a circumference of 95 cm. could be rotated by a motor or by hand, at varying rates, usually so that this was about 1 m. per sec. The colors attached to the drum were exhibited as they passed a slit in a screen. An extension was also made to the drum so that the colors passed by a slit, and the colors only. In other cases with the wheel chronoscope or the chronograph, the black surface of the instrument was seen moving before the colors came into view. The black was as perfect as it could be made, but it suggested a moving surface, which has an influence on the perception.

(4) In order to do away with the moving background and to exhibit the phenomena simultaneously to a number of observers, the stereopticon was

¹ Fullerton and Cattell: *The Perception of Small Differences*, Philadelphia, 1892.

² By J. D. Brown, Camden, N. J. The instrument can be supplied by the instrument maker of the department of psychology of Columbia University.

³ I was so fortunate as to buy three excellent chronographs for \$10 each, they having been made for an unsuccessful system of synchronous telegraphy.

used. The colors were in this case gelatine films attached to a falling screen which dropped before the stereopticon and to a pendulum which swung before it. In this case there appeared on the screen first say green for 50 σ and then in the same area (the size of which could be regulated) red for 50 σ . This method is evidently useful, not only for demonstrating these phenomena, but also for exhibiting colors, words, etc., in various experiments to be made by all the students of a class. I have in this manner also demonstrated reaction-time, muscular fatigue, the effects of mental changes on the pulse and respiration, etc., some of the students making the experiments, and all seeing the results on the screen as they proceeded.

When moving objects are exhibited by the methods described, the resulting perceptions are in some respects analogous to those with the moving eye, and the experimental conditions make possible an analysis and more exact study of the phenomena.¹

If an observer looks at a horizontal window one cm. wide in a black screen, and a black surface behind the screen moves at the rate of one meter per second, white paper and colors attached to the moving surface are seen as they pass by the window in the screen. If a piece of white paper is used 10 cm. in length it is in view for one-tenth of a second as it passes the window. If the eyes do not move, and they cannot make a movement in one-tenth of a second, a retinal field corresponding to the size of the window is stimulated by the light. The physical conditions are the front edge of the light moving over the field in one-hundredth of a second, then the stimulation of each retinal element for one-tenth of a second by the white light, and finally the second edge of the white moving over the field. We have a series of changes in time which might conceivably be followed by a corresponding series of time changes in the nervous system and in consciousness. As a matter of fact we know that the sensation does not exactly follow rapid changes in stimulation. Lights following one another at intervals of one-hundredth of a second fuse, and we should not expect to see the white surface moving over the field, but a fusion of the white and black. We also know that there is a time

¹ I may perhaps be permitted to make in this connection an incidental remark on behalf of experiment in psychology. The chief observations in this paper could have been made by any one at any time, but as a matter of fact they were not made except as the result of a quantitative experimental investigation, continued at intervals for about ten years.

threshold for vision.¹ If a white surface is exposed to view for a very short time it is not seen. When the time is, say, one-thousandth of a second, it is seen as a gray which increases in brightness until the time of exposure is one- to two-tenths of a second, when the sensation is a maximum. As the time of exposure is taken longer the sensation diminishes. Then we also know that after the objective light has been cut off the sensation may be followed by an after-image.

When consequently a white surface is exposed in the manner described, we might perhaps expect to see a field the size of the window, which would be at first gray and after gradually increasing in brightness would fade away. What I in fact see is a field larger than the window, very bright in the middle and shading through gray into black at each side. A process physically and physiologically in time is given in perception as a spatial continuum. This is evidently a phenomenon similar to that already described when with the moving eye objects successively stimulating the same retinal areas are seen side by side.

The experiment can be varied in many ways, and the results can be made more obvious. If the white surface is on one side 5 cm. and on the other 10 cm. in length we have the conditions shown below in Fig. 4. The left hand side of the field of vision is stimulated for one-twentieth, and the right hand side for one-tenth of a second, and the field is of the same size on both sides. The right hand side appears to me, however, to be perhaps 3 cm. broad, about twice as large as the left hand side and much brighter. There is a contrast effect, making the field darker near the middle on the left hand side and at the edges the white fades away indeterminately. In all these experiments a cloud-like or phantom-like image is seen which is difficult to draw or to describe.

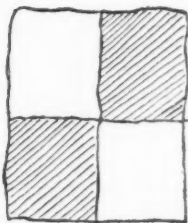
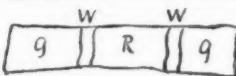
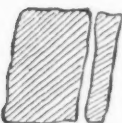
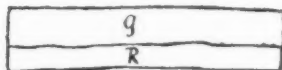
When colors are used—say green for one-twentieth of a second followed by red for the same time—we have the rapid moving across the field of the edges of the colors, and the retinal area stimulated first by red and then by green. The observer does not, however, see first green in the field, followed by a mixture,

¹ Cattell: 'Ueber die Trägheit der Netzhaut und des Sehcentrums,' *Philos. Stud.*, 3: 94-127, 1885.

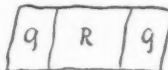
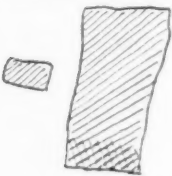
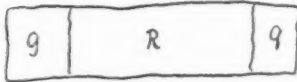
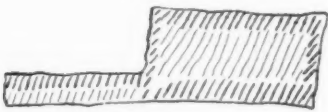
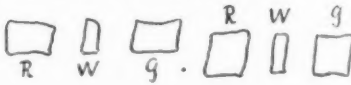
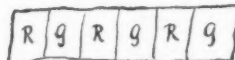
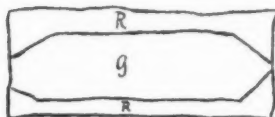
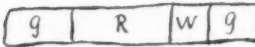
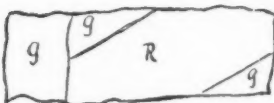
followed by red, but the colors are seen simultaneously on a field larger than the window. I see below green fading away indeterminately into black with bright white in the middle commingling with green below and red above, the red fading into black. The white is brighter than white paper, much brighter than the gray obtained by mixing green and red on the color-wheel. The lines are never sharp, the colors being transfused, the one appearing spread over the other. The appearance of the image changes from time to time; after many experiments I do not now see the extension of the image beyond the size of the window as clearly as formerly, only seeing it distinctly when the moving areas are of different sizes on the two sides.

The experiments on moving objects exposed to view for a short time I have varied in many ways, regarding the rate of motion, the time of exposure, the area of exposure, the number of stimuli, the different colors in varying combinations, the shape of the field, the intensity of illumination, the motion of the field before the stimulus appears, movements of the eyes during exposure and other factors. With so many variables the details become complicated, and it is perhaps best to postpone their consideration, as they do not affect the chief conclusions of the present paper.

The individual variation of the perception is, however, a matter of fundamental importance. With exactly the same stimulus different observers have entirely different perceptions. When the moving field is exhibited for the first time to the observer under the conditions described, he not knowing the character of the stimulus, the perception is usually extremely vague, a mere cloud of light perhaps, the size, shade and brightness of which cannot be described. When the same stimulus is exhibited a second time it is usually perceived more distinctly. The attention has a more definite direction, and the memory image fuses with the effects of the stimulus. After three or four trials the observer ordinarily has a fairly definite perception, and then usually continues to have the same perception as the stimulus is repeated. There may, however, be a sudden change in the way in which the stimulus is perceived. When the observer is told what is actually exhibited or what others see, some



R = Red
g = Green
W = White



Size of Field

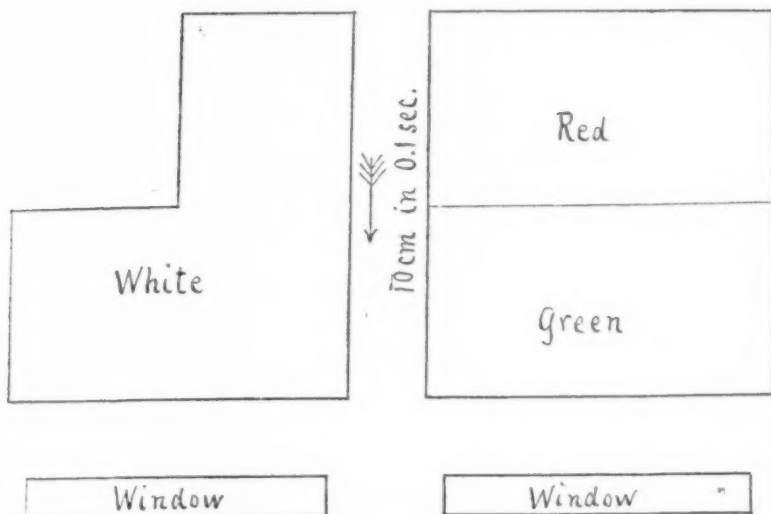


FIG. 4. The stimuli as described in the text are shown above. On the left hand page is shown the way these stimuli looked to ten different observers.

observers have the same perception as before, while with others it is completely changed.

In the accompanying figure the results of the fifth exposure with ten observers and two different stimuli are reproduced. The stimuli are shown on the right hand side of the figure—in the one case a white area 5 cm. broad on the one side and 10 cm. broad on the other, in the second case green 5 cm. broad followed by red 5 cm. broad. The rate of motion was 1 m. per sec. and the field was consequently exposed for 0.1 sec., the area of exposure was 1 cm. wide and 8 cm. long, viewed from a distance of about 30 cm. The drawings of the observers are reduced to one-half. They are more or less schematic, it being difficult or impossible for the observer to draw just what he sees, the outlines being less definite and sharp than the figures indicate.

I have not selected the most variant results, but those of ten advanced students tested consecutively. It is evident that the same stimulus after being exhibited five times is perceived by the observers in entirely different ways. As has already been stated, we have physically lights moving across the field of vision and lasting 0.1 sec. Physiologically we have a limited

field of the retina over which the light moves, and from each retinal element for 0.1 sec. a gradual increase in the commotion of the nervous system which gradually subsides. Nearly all the observers perceive a field larger than is exhibited and the time changes in the nervous system are given in consciousness as a space continuum, *e. g.*, the increase in the commotion of the nervous system as the time of stimulation is increased is perceived as a spatial shading from black through gray to white. But in the area of the figure and in the arrangement of the parts the observers differ completely.

With the stereopticon I have exhibited moving colors on the screen to classes of about thirty students of psychology and have secured results analogous to the above. It is more difficult to reproduce the apparent areas, as the screen is not at the same distance from the observer as the paper on which he draws or at the same distance from all the observers. But the general results are the same. Colors exhibited consecutively are seen simultaneously side by side, arranged and intermingled in very different ways.

These experiments show how absurd it would be to suppose that a perception is in any way a copy of the physical world or the correlate of a simple physiological process. The physical change is exactly the same and the physiological process is nearly the same for all observers, but the perceptions are entirely different. The artificial conditions of the experiment may perhaps give us some indication of what happens to each of us in learning to perceive the physical world as we do perceive it, and may also lead us to consider that there may be more individual variation in the perception of the same physical world than we commonly assume. The variations of different observers and of the same observer at different times indicate that the perception of a series of time changes in the retina as a spatial continuum must be learned by the individual, and may lead us to realize what a complete chaos the visual world must present to the infant.

In these experiments the eye cannot begin to move during the exposure of the stimulus, as the time is one-tenth of a second,

while it takes about two-tenths of a second for the eye to move in answer to a stimulus. If the time of exposure is as long as 0.2 sec. then the eye may move with the stimulus and the field of the retina stimulated may be larger than the window. Zöllner¹ observed that a circle moved under a narrow slit in a screen appears like an ellipse, and von Helmholtz² explains the fact by movements of the eye. He remarks: "Um die Figur erkennen zu können, muss man ihr eben mit dem Auge folgen." But this, as Zöllner himself had previously shown, is incorrect; the phenomenon may occur through movements of the eye or without any movements. If the circle is 5 cm. in diameter and the window 1 cm. in width, we see an ellipse wider than the slit or even a circle 5 cm. in diameter, although the eyes are perfectly stationary. The eyes can be kept stationary by fixing a white spot on the edge of the window; if the eyes move, the movement is betrayed by a motion of the after-image.

If three white squares (whose sides are 2 cm. and which are separated from each other by 2 cm.) are moved across the field of vision at the rate of about 10 cm. per second, and are viewed with the eyes motionless through a window one cm. wide, I see simultaneously the three squares crowded together and

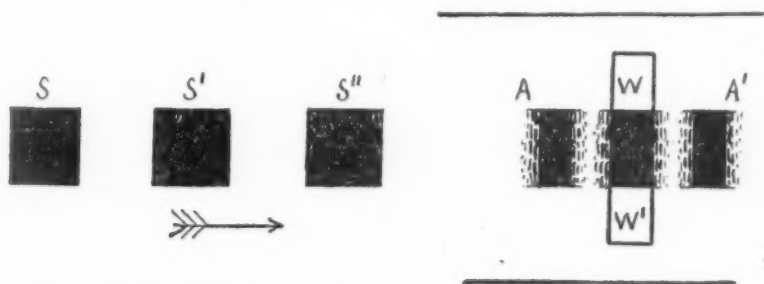


FIG. 5. The squares at the left appear as shown at the right when seen as they pass the window ($w w'$) of the size indicated.

blurred as shown in the figure. If the motion is more rapid the squares overlap, and finally coincide. If the motion is slower the

¹*Poggendorff's Ann.*, 117: 477-484, 1862. Since the present article was written a study of the illusion has been published by Mr. C. C. Stewart (*Am. Journ. of Psychol.*, 11: 240-243, dated Jan., 1900).

²*Physiologische Optik*, 2d Ed., p. 749, 1895.

squares are seen to move across the field, but the perception is not that of one square replacing the preceding on the same field, but of three squares moving along and separated from each other by 2 cm.

These phenomena, again, are similar to those of perception with the moving eye. When the line of vision passes over objects which successively stimulate the same retinal areas we do not see a change in the same field, one object taking the place of the other, but the objects following each other in time are seen contiguous to each other in space. So when objects move and successively stimulate the same retinal area, they may be seen side by side and spread over a larger area than is stimulated. But there is the difference, already discussed, that with the moving eye there is no fusion or blurring, however rapid the change in stimulation, and the space relations of the objects remain the same whatever the rate of movement, whereas when the objects move there is partial fusion when the rate is 10 cm. per second, and the apparent space relations vary with the rate of motion.

The phenomena described for vision also hold for touch. If the finger is moved along a rough edge, as the teeth of a saw, the same points of the skin are touched consecutively, but an extended object is perceived. Here, as in the case of vision with the moving eye, we have the muscular sense as a factor in the total perception. But when the finger is stationary and the edge moves over the finger we have the perception not so much of one touch after another on the finger as of an extended moving object. In this case the illusion that the finger is moving is very strong.

Some of the results of this research may be summarized as follows:

(1) When objects are moved slowly over a limited visual field, while the eyes are stationary, they are not seen one after the other at the place where they are exhibited, but seem spread out over a larger area than can be seen. When successive white or colored surfaces pass over a limited field of vision so rapidly that the eye cannot be moved while they are in view, the stimuli do not seem to follow one another, but are perceived

simultaneously side by side, variously arranged and commingled. What is physically a stimulus lasting a certain time and physiologically a series of changes in time is psychologically an area in which the colors are spread out side by side, and the processes in time are a spatial continuum. The colors appear in this case to cover a larger area than is exhibited.

(2) Observers differ greatly in their perception of exactly the same physical stimulus under these artificial conditions. On the first exposure there is usually no definite perception, but as the experiment is repeated the observer tends to perceive the stimulus always in the same way, although two observers rarely or never have the same perception. The perception with some observers alters greatly when they know what the stimulus is or what others see; with other observers it is not altered. These phenomena indicate that visual perceptions are built up by the individual in the course of experience and may differ greatly in different individuals. For the infant the visual world is probably a chaos.

(3) When the eyes are moved so that the line of sight passes over objects, we have one after another stimulating the same retinal field, but we perceive the objects simultaneously side by side. A series of physiological processes in time is perceived as a spatial continuum. The same phenomena obtain for touch. In the case of vision the changes in the field may be very rapid—more than one thousand per second—and yet there is no fusion. We have, however, movement of the field and partial fusion when the head is shaken. The different results with the moving eye and with objects in motion (the retinal processes being the same) prove that the phenomena of fusion and color-vision are cerebral and mental rather than retinal.

(4) In general these experiments show that perceptions are not 'copies' of a physical world or the 'inner side' of special physiological processes, but are in large measure the result of experience and utility. The phenomena of vision as a whole require this conclusion, but the facts here described, showing that changes in time are perceived as extension in space, appear to be of fundamental importance for psychology and for epistemology.

JUDGMENTS OF MAGNITUDE BY COMPARISON WITH A MENTAL STANDARD.

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AND DR. EDWARD THORNDIKE,

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This paper presents the results of an experimental study of judgments of magnitude. The special aim was to discover the relation between the accuracy of these judgments and the amount judged when the basis of judgment was a mental standard. The judgments, that is, were such as one makes in ordinary life when he estimates the weight of a package as so many pounds, or the length of a table as so many inches.

The bulk of psychologists' investigations of the accuracy of our judgments of magnitudes has sought quantitative estimates of the amount of physical *difference* required in various cases to bring about a judgment of *difference*. Moreover the treatment of the results has been generally carried out with the intention of discovering *quanta* of sensation. The idea of an arithmetic of sensation units has often pervaded such studies to the exclusion of other real issues. It has been supposed that our judgments of difference in the intensity or the magnitude of two stimuli are the direct consequences of the presence in one case of an added quantum of sensation. If a subject choosing a weight as equal to a weight just lifted, made an average error of one-twentieth the amount, it has been supposed that he did so because the addition of a physical difference of less than that failed to give him any or a sufficient quantum of sensation. Fullerton and Cattell have shown the superficiality of this view.¹

If we look at our judgments of difference we find that they may be based on a feeling of shock or tension felt in passing

¹ *The Perception of Small Differences*, 1892.

from one stimulus to another or on a similar shock of difference felt in passing from the associates called up by one to the associates called up by another. We may judge two lines to be unequal by the direct passage from one to the other, or we may have learned to call one twenty and the other twenty-one centimeters and may judge by the comparison of the two verbal associates. The *conditio sine qua non* of discrimination would seem to be the ability of the neural processes caused by the two stimuli to form connections with neural processes differing enough to give rise to a conscious judgment. If by associative connections the original processes can serve as excitants to more and more differing brain processes, it makes no odds whether *per se* they could give rise to a judgment of difference or not. And if such neural connections have been formed, the consciousness originally attending the first links may decrease or vanish without altering the success of the judgment. Attention may shift entirely from the bare sensation given. Feelings of its intensity or magnitude may play a very small part in the final judgment.

Let us now take the case of a judgment, not of difference or equality between two stimuli, both readily accessible, but of magnitude in terms of some fixed unit not present to sense at the time. There is here no occasion for the feeling of direct sensory shock. Accordingly such judgments give us the means of determining the process of discrimination in a case where it is due to the associations. The mental process that occurs is this; one lifts a weight or looks at a line or an area or makes a pull or looks at an illuminated area and feels "It is 260 grams, or 140 mm., or 1 candle power," as the case may be. This feeling may seem satisfactory, or one may feel, in addition, "No! that is probably too much or too little; it is so and so." There is no conscious comparison with anything. Your judgment of the magnitude of the stimulus is like your judgment as to whether a certain object is a dog, horse or what not. The only difference is that you have a series of possibilities instead of a few alternatives. Of course, you may never reach a perfect association between the sense impression of the stimulus and the image of the words denoting its true magnitude, but may judge within a limit of error.

It seemed worth while to study the accuracy of such judgments because of their commonness in practical mental life, because of the possible value of getting at the facts on which Weber's law was based and by which it has since been disproved, from a new point of view, and because the naturalness of such judgments may make them better suited for individual tests than the artificial judgments necessary in tests by the methods of least noticeable difference and right and wrong cases. A number of minor points were brought out by the experiments which throw some light on the mental processes involved.

The method used (unless a special statement is made) was to guess at the length, or size, or weight of certain test weights, lines and areas, then to note the true magnitude and record the error. The weights, lines and areas used were intended to be indistinguishable in any other way than by their magnitudes. Precautions were taken which prevented the subject from knowing by the order of succession, or by the record he kept, which magnitude to expect or not to expect.

JUDGMENTS OF WEIGHT.

A series of test weights ranging from 200 to 500 grams by steps of 5 grams was used with subjects *H.* and *WH.* Each weight was, in the course of the experiment, lifted by *H.* ten

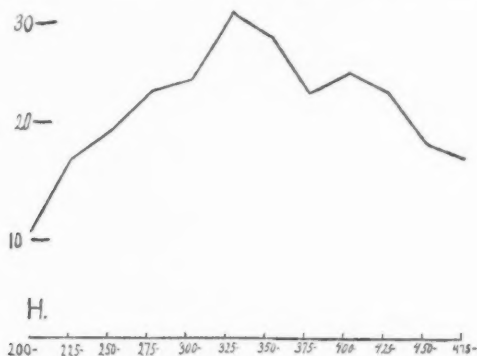


FIG. 1.

Judgments of weights. Subject, *H.* The ordinates represent the average error in grams. The figures along the abscissa represent the magnitude judged, *e. g.*, 200- equals 200 to 220 inclusive.

times, making 600 determinations in all. Before starting on the series the subject was given a practice period of 100 determinations, in order that the first few weights lifted in the test series might not be at a disadvantage.

The 310- and 335-gram weights were omitted in this experiment. Grouping each 50 judgments together and taking the average of the errors and plotting the curve we have the result shown in Fig. 1. Both constant and variable errors are included.

The shape of the curve is due partly to the fact that judgments at the extremes are freed from certain possibilities of error, but partly to unknown influences on the subject. He did not consciously judge by comparison with the extremes, as one might suppose.



FIG. 2.

Judgments of weights. Subject, *H.* Comparison of the two halves of the series.

In Fig. 2 are shown (1) the average error and the variable error¹ for weights 200 to 350 and 350 to 500, (2) the error as it

¹ In general in these experiments the constant error is negligible or affects large and small magnitudes alike. No account is taken of it in this report except where its presence might cause an apparent inaccuracy, in large judg-

should be according to Weber's law and (3) the error as it should be according to the law of the combination of errors (the error increasing as the square root of the magnitude).

W. H., after a practice period of 50 determinations, judged the series of weights, making three judgments of each. Fig.

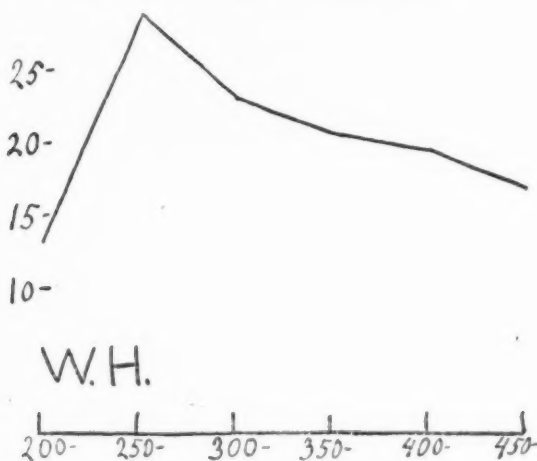


FIG. 3.

Judgments of weights. Subject, *W. H.* The ordinates represent the average error in grams.

3 represents the average of his errors for the first ten weights, second ten, etc. Fig. 4 represents (1) his average error and variable error, for weights 200-350 and 350-500, (2) the error as it should be according to Weber's law and (3) the error as it should be according to the law of the combination of errors.

Subjects *W.* and *T.* were tested with a sense of weights ranging from 40 to 120 grams by steps of 5 grams. *W.* estimated each weight 50 times, and *T.* estimated each weight 100 times. Fig. 5 shows the relative inaccuracy of the judgments, the heights representing the average error, including the constant as well as variable error. No essential difference appears in a curve plotted from the variable errors alone. Obviously there is a tendency in *H.* to over-estimation of weights helped him with the heavier weights. We can discount this help roughly by grouping all his errors in judgments of weights from 200 to 350 grams, treating them as one series and finding the variable error, and then doing likewise with the errors of the judgments of weights from 355 to 500.

is here no correspondence whatever with Weber's law. The dotted line shows the error according to the law of the combination of errors.

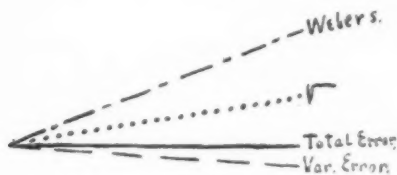


FIG. 4.

Judgments of weights. Subject, *WH*. Comparison of the two halves of the series.

Subject *T.* was tested later with a series running from 420 to 500 by steps of 5 grams, ten determinations being made with each weight. The average error of this series is compared with that of the first 10 determinations of the weights from 40 to 120

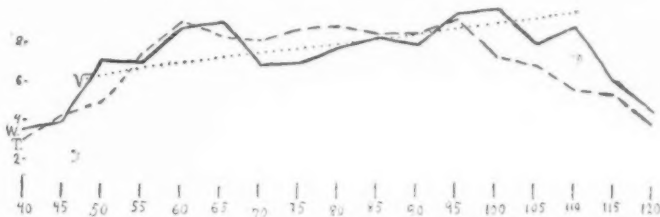


FIG. 5.

Judgments of weights. Subjects, *W.* and *T.* The ordinates represent the average error in grams.

in Fig. 6. Nothing definite can be said about the comparison, however, as the error is so large in comparison with the extent of the series.

These experiments with lifted weights are unanimous in showing no agreement whatever with Weber's law, and also in

showing if taken roughly a less rapid increase of inaccuracy than is in accordance with the law of the combination of errors. Probably in a new variety of experimentation like this some dis-

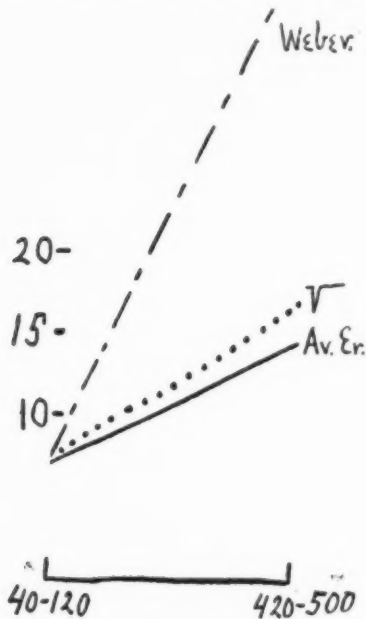


FIG. 6.

Judgments of weights. Comparison of series 40-120 with series 420-500. Subject, *T*.

turbing factors entered which the writers failed to note. However, we think these are slight.

JUDGMENTS OF LENGTH.

The apparatus for testing the accuracy of judgments of length consisted of a series of slips of paper of the same sort and equal width, of lengths of from 100 to 300 mm., varying by steps of 2 mm. There were two of each length. Subjects *H.* and *T.* were tested in the same manner as with weights. Fig. 7 presents the results of the experiments. The curve was plotted from the average errors of the estimates of lengths 100 to 118, 120 to 138, etc. *H.* had the knowledge of lengths due to having made the apparatus and the practice of ten es-

timates. *T.* had the practice of twenty estimates. *H.* judged directly from the gross length. *T.* consciously marked off 100 or 200 mm. with his eye and judged the remainder. As

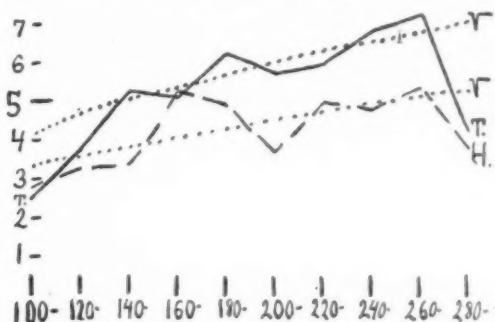


FIG. 7.

Judgments of length. Subjects, *H.* and *T.* The ordinates represent the average error in millimeters. The dotted lines are curves of the error as it would be if it followed the $1/\sqrt{}$ law. The continuous line is for *T.*'s error, the dash line for *H.*'s.

before, there is no approach to coincidence with the inaccuracy demanded by Weber's law.

JUDGMENTS OF AREA.

The judgments of area were made with the following apparatus: a series of parallelograms ranging from 10 to 140 and 190 to 280 sq. cm., varying each from the next by 1 sq. cm. Their proportions were almost the same (no one of them could possibly be distinguished by its shape). For example, the dimensions of those from 137 to 175 sq. cm. were

15	$\times 9.133\frac{1}{3}$
15	$\times 9.2$
15.1	$\times 9.2 +$
15.1	$\times 9.275$
15.15	$\times 9.31$
15.15	$\times 9.375$
15.2	$\times 9.41$
15.2	$\times 9.475$
15.2	$\times 9.54$

The error in area due to error in measuring and cutting them was tested roughly by superimposing, re-measuring and weighing them. It was always less than 0.1 sq. cm. in those

from 10 to 140 sq. cm. and always less than 0.2 sq. cm. in those from 190 to 280 sq. cm.

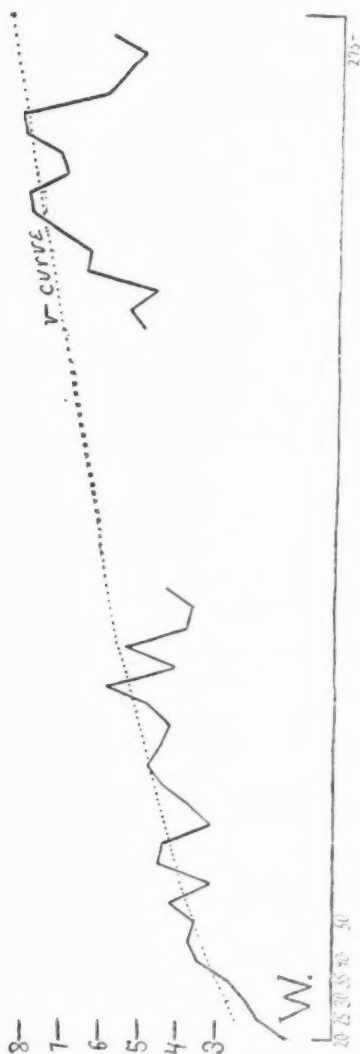


FIG. 8.

Judgments of area. Subject, *W*. The ordinates represent his average error in square centimeters.

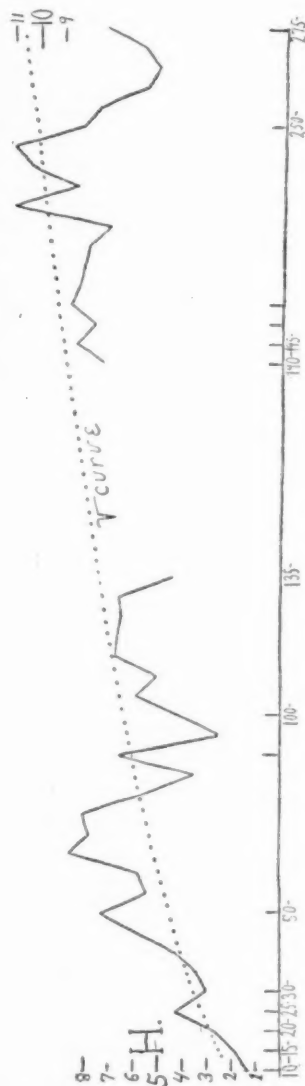


FIG. 9.

Judgments of area. Subject, *H*. The ordinates represent his average error in square centimeters.

As in the other experiments, the subject picked out an area at random, judged its size, looked to see the real size and recorded his error.

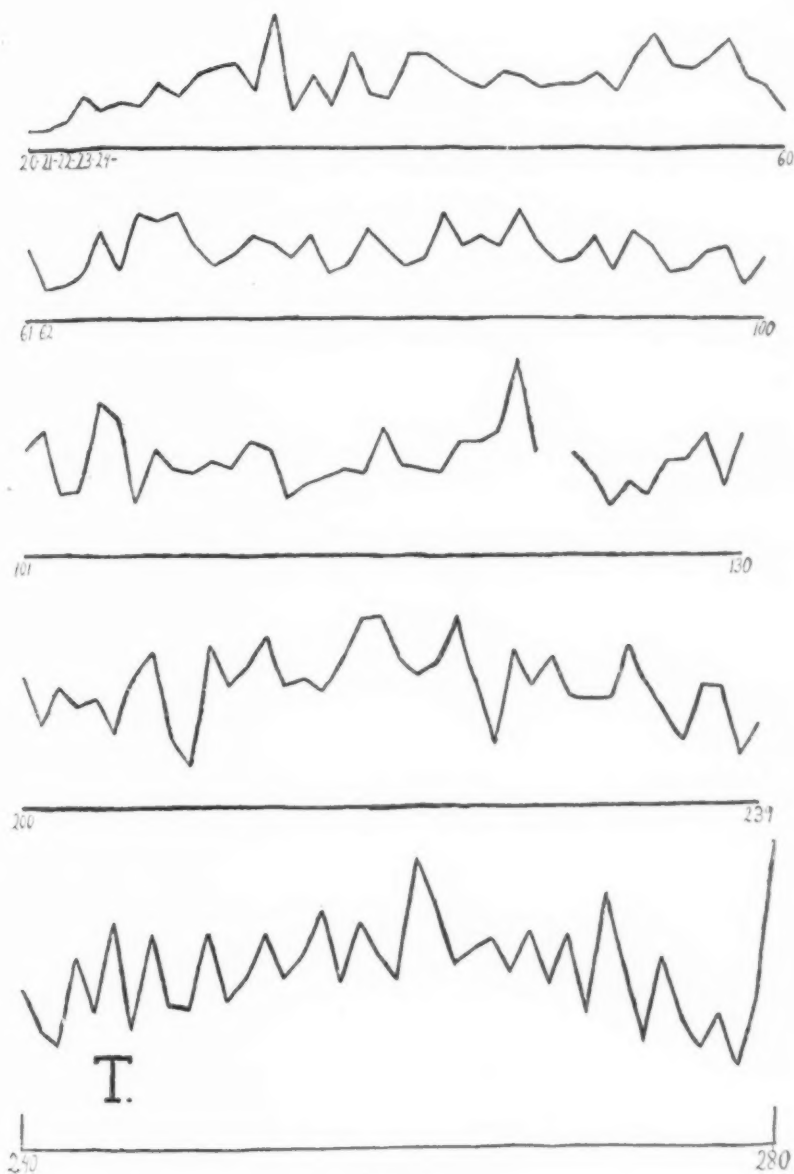


FIG. 10.

Judgments of area. Subject, *T*. The ordinates represent his average error in square centimeters.

After a practice period of fifty judgments *W.* made estimates of the areas from 20 to 140 and 200 to 280 sq. cm. In all, ten estimates of each were made. The work covered several days, but as elsewhere no one size was favored by practice, freshness or special effort.

Fig. 8 shows *W.*'s average error for areas 20-24, 25-29, 30-34, etc. The dotted line represents what the error should be in accordance with the law of the combination of errors. Leaving out of account the increased accuracy at the ends of the series, the actual errors follow the theoretical law fairly closely. There is evidently no agreement at all with Weber's law.

H. was tested with the full series 10-140 and 190-280 sq. cm. Five estimates of each were made. Fig. 9 shows his average error for areas 10-14, 15-19, 20-24, etc.

T. was tested with a number of smaller series one at a time. Ten estimates of each area were made. Fig. 10 shows his average error for each area. The series were the following and were given in this order: 20-60, 240-280, 61-100, 101-140, 200-239. These records are interesting in that they show no rise within any series, no marked rise save between 101-140 and 200-239. The slow increase from 60 to 140 in the

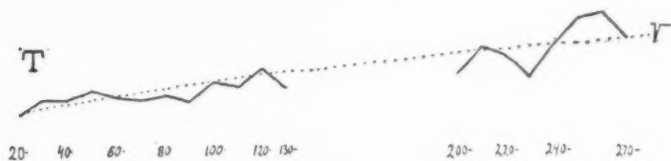


FIG. 11.

Judgments of area. Subject, *T.* A condensation of figure 10.

case of *H.* is paralleled here. The marked and extended increase in accuracy at the ends shown in the longer series is not found here. Fig. 11 shows *T.*'s errors when averaged for each ten successive areas.

It seems fair to conclude from these experiments as a whole (1) that the absence of any correspondence with Weber's law observed by Fullerton and Cattell in judgments of direct comparison holds true of judgments of comparison with a mental

standard. No subject in any of the experiments showed anything like any such increase of inaccuracy as Weber's law implies. Besides this, (2) the experiments show that in such judgments as we ordinarily make in life there are many factors besides the magnitude of the thing judged which affect the accuracy of the judgment, otherwise the form of the curve would not vary so much with different subjects.

A NEW EXPLANATION FOR THE ILLUSORY MOVEMENTS SEEN BY HELMHOLTZ ON THE ZÖLLNER DIAGRAM.

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Every one who has given any considerable attention to the Zöllner illusion is familiar with the strange gliding movements of the vertical columns which, under special circumstances, are

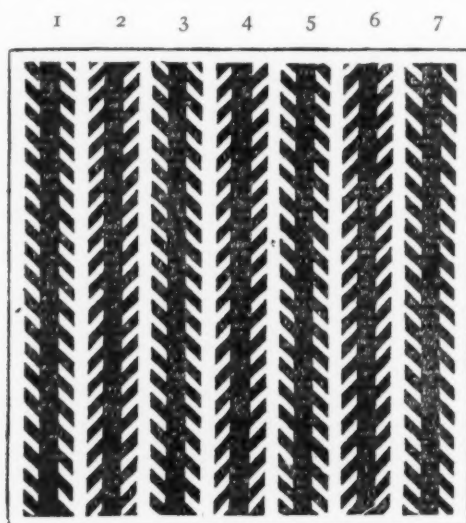


FIG. 1.

to be seen on the heavy-line pattern here figured. (Fig. 1.) Helmholtz was the first to describe these movements.¹ According to him, they are to be seen where one fixates the point of a needle moved horizontally across the diagram. If one be sufficiently practiced in following steadily a moving point a most striking and unusual phenomenon is presented. The vertical

¹ *Physiologische Optik* (2), 712.

columns are seen to shift their position with a graceful gliding movement in the direction of their length. Those verticals that bear upward-running transversals glide in one direction, while those with downward-running transversals pass as rapidly in the opposite direction. If the movement of the fixated point be from left to right, the former columns (2, 4 and 6 of Fig. 1) dart upwards, the latter downwards, just the reverse being the case when the moving point goes from right to left. Moving the fixated point alternately back and forth, and taking care to maintain an appropriately moderate rate of speed, there results a state of 'strange unrest' over the whole diagram, totally unlike anything else in the whole realm of geometrical optical illusions. One is reminded most forcibly, perhaps, of what might happen if the illusory creeping motion of the threads of rapidly rotating endless screws were presented to the eye in seven parallel lines and with alternate differences of direction. Not every one gets this illusion directly, but a brief practice in moving fixation, as well as some attention to the most favorable rate of movement and distance of diagram from the eye, will reveal it in all its vividness.

Now as to the existence and general nature of this illusion there can be no question. The apparent slippings and slidings of the columns are incontestable. It is only as to their *explanation* that there may be disagreement and discussion. It is accordingly the purpose of this paper to offer an entirely new explanation for these phenomena, an explanation which is based upon a consideration of the peculiar manner of stimulation experienced by the retina as the fixated eye passes over the diagram. That is, the explanation here given will be couched wholly in retinal terms, and not in terms of any elementary geometrical illusions which arise under the given conditions and which themselves cause the phenomena in question.

But let us see first what explanations are in the field. I have been able to find three: (a) Helmholtz's own is as follows:¹ The path of the moving eye is along a line which makes an acute angle with the oblique transversals which it crosses; or, more generally stated, every sensitive point of the

¹*Loc. cit.*, 712.

retina is moving in a direction oblique to the actual obliques. There results a multitude of ideal acute angles, or "direction-differences," and since acute angles are always enlarged for perception, or what amounts to the same, since direction-differences are always magnified by contrast, the numerous transversals must appear to swing in such a way as to express this illusory angular enlargement. The upward-running transversals will consequently swing upwards, the downward-running downwards, and since any given system of transversals will move in concert, this illusory movement must be communicated to the vertical strips themselves. This is the substance of Helmholtz's explanation. It rests, plainly, upon a supposed angular illusion, the production of which draws the further illusion in its wake. Some of the minuter considerations, which in Helmholtz's view go to support this explanation, must be carefully examined later.

(b) Thiéry¹ claims to find the cause of these movements in the equivocal character of the prism surfaces upon which, to many observers, the vertical strips of the Zöllner pattern seem to be projected. To those gifted with the power to see perspective effects in linear drawings, these prism surfaces appear to tilt alternately backwards and forwards, and thus, on the perspective theory of optical illusions, the ordinary Zöllner illusion is produced. In the usual forms of the figure the intersections of these prism surfaces must of course be imagined lines lying in the *white* verticals between the black columns. Now these edges of intersection being imagined, as the eye moves horizontally over the figure, each successively fixated edge, Thiéry says, steps into the foreground—as fixated points or lines of equivocal figures always do—and thus each imagined intersection becomes in turn the front edge of a prism. By an immediate consequence the adjacent intersection on either side must retreat into the background, only to step forward once more when the moving fixation-point has reached the next intersection. It is this ceaseless movement backwards and forwards, consequent upon the movement of the eyes and due to the equivocal nature of the figure, that to Thiéry's mind is responsible for the 'strange unrest.'

¹ *Phil. Studien*, 1895, XI., 320-321.

Filehne¹ seems to entertain an identical opinion of the matter, though the expression of his opinion is nowhere explicit.

(c) Judd² adopts still another point of view by attempting to apply the observation of Müller-Lyer that if an acute and an obtuse angle have equal legs, those of the obtuse angle seem longer. Now, he says, the transversals of the Zöllner pattern make both acute and obtuse angles with the intersected verticals, and the horizontal movement of the eye, allowing, as it does, the successive fixation of the various points of the figure, permits the successive false estimations of the sides of these angles to come into prominence. Of course the mal-estimations that are effective for this illusion are those connected with the legs formed by the verticals themselves. And the particular slope of the transversals of a given column will determine the direction, up or down, in which this column will appear to move.

It will be seen that Helmholtz alone attributes any influence to the imaginary line drawn over the figure by the moving eye. The other two writers look upon the eye's movement merely as necessary to bring out characteristics latent in the figure, the conditions being thereby supplied for the arising of the further illusion of the gliding columns.

It is really remarkable that this curious illusion of motion has received so little serious attention. Either because this is less easy to see than the usual illusion that has engaged the almost exclusive attention of observers, or because the weight of Helmholtz's authority has tended to confine explanatory attempts within a particular realm of spatial phenomena, the true cause of this illusion, simple and near at hand as it is, has been persistently overlooked.

AN EXPLANATION IN TERMS OF PECULIARITIES OF RETINAL STIMULATION.

Suspicion against the current explanations may be readily aroused by noticing that the illusory movements are only faintly perceived upon the *light-line* models of the Zöllner figure. Attention is always directed to the original heavy-line diagram,

¹ *Zeitsch. f. Psychol.*, etc., 1898, XVII., 47-48.

² *PSYCHOLOGICAL REVIEW*, 1899, VI., 260.

when this matter is under discussion. This would seem at once to indicate that purely retinal influences are powerfully operative in determining at least the vividness of the illusion.

My own suspicions were first awakened by accidentally noticing what happened when, with the diagram in full view, the convergence of the eyes was unthinkingly relaxed. As the eyes diverged the illusory movements began. They occurred again while the eyes were returning to converge upon the diagram. And if, when the optical axes were nearly parallel, there were slight movements to a state of greater or less divergence, the columns exhibited on their part their appropriate shiftings. In themselves, of course, these observations contributed nothing decisive to the problem in hand, but they served to arouse a line of investigation which has not been altogether without positive results.

It may be said here that for ease of observation a copy of the Zöllner pattern, like that of Fig. 1, should be pasted upon a small piece of cardboard of convenient size for handling. With this device one may demonstrate the Helmholtz phenomenon much more readily than by the usual method. Holding the column in any desired position—vertical, horizontal or at any angle—it is only necessary to shake the diagram slightly back and forth in its own plane and in a direction perpendicular to that of the columns to produce very vivid effects. The ‘shaking’ of the diagram is essentially the same as moving the eyes over it, for the eyes cannot readily follow its motions and consequently the image of the diagram moves over the retina, the result being the same for perception as if the retina moved over the diagram. The relations between objective and illusory movements in respect to their directions are the same with this as with the usual method of producing the illusion. The slippings and slidings of the columns to be seen under these circumstances strongly suggest that we are in the presence of phenomena analogous to those of the ‘Fluttering Hearts.’ There is a similar jelly-like movement here, but instead of a dim illumination the full daylight is requisite for the best effects. That the observation of the illusion is possible by the method just described would seem at least to show that Thiéry’s explanation in

terms of the equivocal perception of prism-faces cannot find any reasonable application under the conditions here in force. No trace of such equivocal swingings of the prism surfaces is to be discovered.

Further, it becomes hard to reconcile the theory of Helmholtz with the observation that *the columns move in such a way that the obliques are everywhere parallel to their original direction*. That is, if the apparent movement of the columns is due to a principle of direction-contrast working upon the oblique transversals, or, in other words, if this movement is due to the illusory enlargement of acute angles successively formed by ideal lines passed over by the moving eye, there should be some vestige of a *rotary* motion observable on the obliques. For angular increase must take place about a vertex as center of rotation. And if such angular increase is the underlying cause of the phenomenon under consideration, it surely must be possible to perceive some *twist* in the transversals, some slight departure from their original direction. But as a matter of fact no such thing is to be seen. The columns move smoothly and evenly upwards and downwards, the obliques never changing in the slightest the course of their original slope. They rotate neither about the point of intersection with the verticals, nor about either of their ends. They move rather as if impelled by some push given to the verticals, to which they seem rigidly attached. If correct, this observation most certainly discredits Helmholtz's explanation.

And, finally, Judd's explanation in terms of mal-estimations of sides of angles is just as little able to maintain its claims. *For the verticals are not at all necessary for the illusory movements*. By cutting suitable strips from striped cloth or from properly ruled paper, the Zöllner pattern may be reproduced *without its verticals*. Under these circumstances the Helmholtz illusion persists unchanged in all particulars. But Judd's explanation is no longer applicable, for the sufficient reason that there are now no acute or obtuse angles whose sides are to be successively mal-estimated as the eye moves over the diagram. The main lines which supplied the indispensable condition for this explanation are no longer present, and it seems hardly

reasonable to suppose that imagined verticals may take the place of the actual lines that have been removed.¹

It being now evident that no one of the current explanations can square with the simplest facts of the case, let us attempt the statement of the purely retinal theory promised above. Consider, for convenience, a single column of upward-running

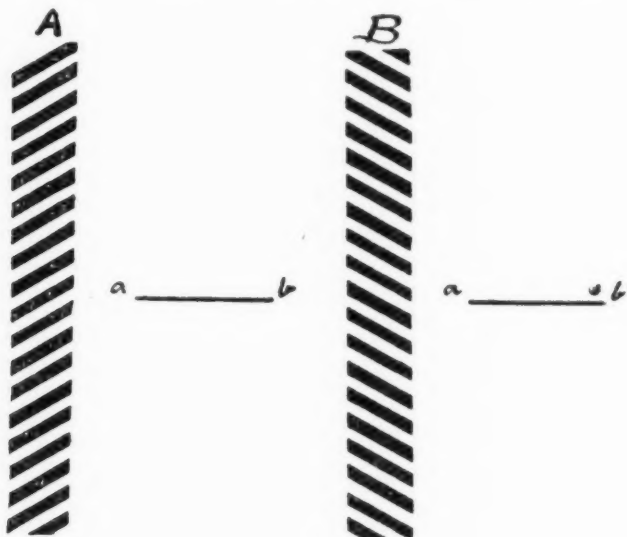


FIG. 2.

obliques without the central vertical (*A*, Fig. 2), and let the eyes be supposed to follow a point moving horizontally from left to right. That the illusion persists with only a *single* column may be easily ascertained by carrying out the usual movements of the eyes after all but one of the columns of the reg-

¹The observation of Witasek (*Zeitsch. f. Psychol.*, etc., 1898, XIX., 153) to the effect that he observed no movement of the columns when the obliques alone were presented to one eye, though such movements were continually being observed on the complete diagram upon any slight movement of the eyes, was probably due among other things to the lightness of the lines he was observing. He was making a study of the usual Zöllner illusion under the conditions of stereoscopic combination—the verticals being presented to one eye and the obliques to the other—and under these circumstances the attention was presumably too fully occupied with other matters to notice any faint illusion. If, however, the eye had been moved intentionally over the part bearing the obliques only, the illusory movements could not have escaped his notice.

ular pattern have been covered. As to the position of the fixated point, experience shows that the clearest perceptions emerge when it moves along a line situated at the side of the column, along the line *ab*, that is, in Fig. 2. For my own eyes the most satisfactory results are obtained when this line is at the *right*. Others may find that the left side is preferable. In any case indirect vision seems best adapted to the perception of the illusion.

What happens under these circumstances, as the eyes follow the fixation-point along *ab*, may best be seen by considering the experience of a single vertical line of retinal elements. Let this line be thought of as resting on the extreme left edge of the column before us. It will then receive along its length alternate excitations of black and white. Now let the eyes move. As the supposed line of retinal elements passes from the left to the right edge of the column, the alternate excitations of black and white will fall higher and higher upon the line. The practical result for perception is therefore exactly as if numerous stimuli had *moved* up over a resting retina. This line of elements has experienced the full conditions for the perception of motion. But what happens to the single line happens in like manner to all vertical lines of elements that pass over the column of obliques, the upward-creeping excitations on each succeeding line being in each case lower down than those on the line ahead, as is of course determined by the slope of the stimulus-giving obliques. Manifestly the visual effect of this is entirely similar to the moving of the column bodily upwards over the resting eye.

If now the eyes move from right to left, the vertically arranged spots of stimuli, as we have pictured them, will travel downwards, and the visual appearance will be that of a descending column of obliques.

The directions of these vertical movements, it will be noticed, are in exact accord with the observations of Helmholtz. Columns bearing upward-running transversals run upwards when the eyes move to the right, downwards when the eyes move to the left.

Exactly the same style of considerations is to be applied to

the columns of form *B* (Fig. 2). With a movement of the eyes to the right successive stimulations will fall upon lower and lower points of the retina and the column as a whole will appear to move downwards, just the reverse being the case when the eyes move to the left. This again is wholly in accord with Helmholtz's observations.

The conclusion of the whole matter then is simply that the illusion under discussion is caused by the peculiar manner in which stimulations travel upon the retina. The horizontal movement of the eyes across lines lying oblique to their direction is equivalent in retinal terms to an ascending movement of the entire set of obliques over a resting retina. For perception the two processes have identical effects. Hence the illusion.

The general correctness of the view just expounded may be vividly brought out by the following procedure. Taking any piece of cloth or paper with closely lying stripes and placing upon this a piece of cardboard in such a way that the stripes run upwards across the vertical edge, move the cardboard in a direction perpendicular to its edge, the eyes meanwhile following some point on the latter. The effect will be that of an apparent ascending of the portions of the cloth successively uncovered by the moving cardboard. What occurs here is essentially the same as in the typical cases. Stimulations are mounting higher and higher along the vertical lines of the retina, and hence all the usual conditions for the perception of upward motion are fulfilled.

If the above explanation is correct, several important corollaries follow immediately from it:

1. The *rate* of the illusory movement must depend upon the rate of the horizontal movement of the eyes. Helmholtz does not fail to notice this fact,¹ though he simply mentions it without discussion. It may be readily verified on the regular 'pattern' or upon isolated columns of obliques. Now on the theory of moving retinal stimulations this coincidence of rates *must* occur. The more rapid the horizontal movement of the eyes, the more rapidly will any bit of stimulus traverse its path from end to end of any oblique, and consequently the more illusory speed will the vertical column seem to possess.

¹ *Loc. cit.*, p. 713.

2. The *excursion* of the illusory movement must depend directly upon the *slope* of the oblique. This again is evidently a direct consequence of our explanation, for the vertical distance from tip to tip of each oblique increases with the slope of the latter, and accordingly the amount of vertical movement of the various stimulations along any retinal line, such as we have supposed above, must vary directly with the

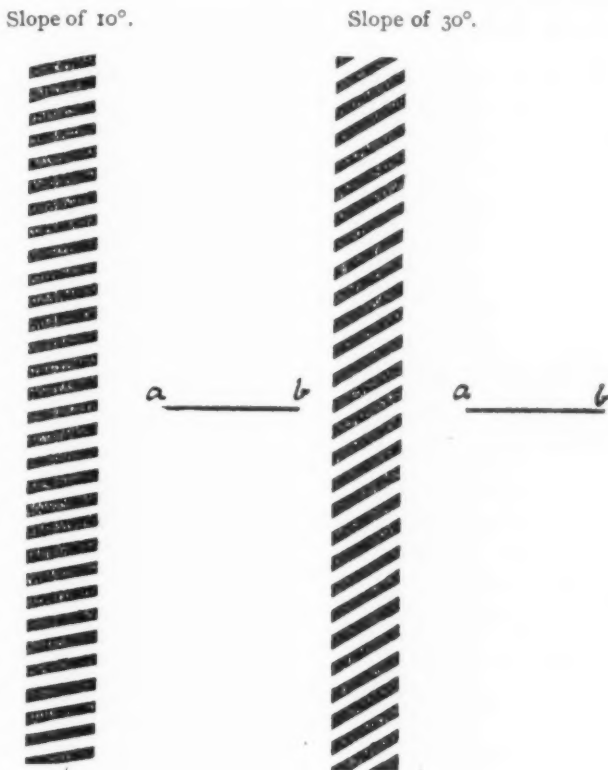


FIG. 3.

tilt of the oblique. To subject this deduction to the experimental test, cards were prepared bearing columns whose obliques sloped at angles of 10° , 20° , 30° and 40° respectively. At the side of each card, in the position indicated in the accompanying figure (*ab*, Fig. 3) a slit was cut which allowed the introduction from above of a common pin. The head of

the pin, moving upon the surface of the card, served as moving fixation-point for the eye. The length of each slit was 18 mm. Naturally the results obtained do not admit of quantitative statement, but *the anticipated increase of the illusion with the increase of the slope was everywhere realized*. The most striking way to bring this into evidence is to 'shake' the cards, in the manner described on page 360, the columns being held horizontally. There is no question that the columns whose obliques have a slope of 40° appear to traverse a much greater horizontal distance than those with 10° obliques.

Now it is difficult to see how Helmholtz's explanation can adequately account for this fact. Indeed it would seem that Helmholtz must expect exactly the reverse of what we have found. For in accordance with his principle that clearly recognizable differences are estimated as greater than those less clearly recognized,¹ it would seem that the angular enlargement due to direction-contrast would be greater for a slope of 10° than for one of 40° . For in the former case the difference of direction between the obliques and the imaginary paths traced by the eye must be clearer than in the latter case. Or, if not clearer, at least of a similar grade of clearness, which should cause the illusion to be equally great in the two cases. But it is unnecessary to attempt to strain Helmholtz's explanation into any form which shall pretend to cope with these facts in regard to the amount of the illusory displacement of the columns. These facts fall so readily into line with the retinal explanation proposed that they form one of its strongest confirmations, just as their absence would completely overthrow the theory here defended.

(c) Again, on the view here presented, the illusory movements should progress smoothly, without the slightest change of inclination on the part of the obliques. We have already emphasized the fact that this is so. And, in addition to the above, any slight movement of the eyes along the line of fixation should give a correspondingly slight illusion. And movements of the eyes by *stages*, that is with momentary stops between short movements, should be accompanied by an exactly parallel be-

¹ *Loc. cit.*, 705 and 714.

havior of the columns. Both of these deductions are unmistakably verified. Especially may one see what is meant here by substituting for the column of Fig. 3 a rather large patch of oblique lines. The movement of the eyes by tiny stages makes the patch appear to move stealthily up and down, creeping slightly along, then resting, then creeping further, etc. The whole impression during such an observation as this is that of some set of visual stimulations making their way gradually up and down the retina. Of the rotation of the obliques, which if it existed should assuredly become evident with these relatively long lines, not a trace is to be observed. There is the same smooth upward march that is to be seen in the case of the usual columns.

Another way of demonstrating this same thing is by diverging and converging the eyes upon the diagram as described above. This experiment may be varied by taking a single column from the 'pattern.' As the axes of the eyes diverge towards parallelism two columns are of course seen moving in opposite directions. But what interests us here is that every chance alteration in the degree of divergence produces a corresponding movement of the columns; slow, if the change in the eyes is slow, and of short excursion, if the change of divergence is slight. In short, when the eyes move there are immediate response and complete correspondence on the part of the moving column.

Now these facts can hardly be made harmonious with the thought of direction-contrasts that develop as the eyes move. Surely some length of movement must be requisite for the development of the consciousness of such direction-contrasts, and one should expect on this hypothesis, not the smooth movement that we have observed, but rather the progression of the columns by jerks, as the ideal horizontal line became long enough to evoke a feeling of difference in direction. Such jerky movements might not reveal themselves under the usually observed conditions of continuous eye-movement, but it would seem plausible at least to expect them under the special conditions here in force, were Helmholtz's explanation correct. The entire absence of such unsteady motion in the illusion, and the total impossibility

of discovering the faintest trace of rotation on the part of the obliques, combine to form a strong bit of evidence against the explanation here combated.

(d) That columns of upward-running obliques should run upwards, and that columns of downward-running obliques should run downwards, is an inevitable deduction from our premises.

Now Helmholtz records this fact with perfect clearness, but for some strange reason he does not attempt to show why either sort of column should have *its peculiar direction* of movement. And indeed on his theory this would be an awkward task. For instance, consider a single upward-running oblique. If the imaginary path, which is to establish the direction-contrast, be supposed to cut the oblique at its point of intersection with the vertical, the oblique must execute opposite movements with its two ends. With the eye passing to the right, the left end of the oblique must move down, the right end up. Which of these two movements is to be decisive for the final movement of the column? The one last seen? But why should not the movement first seen be just as effective, and consequently why should not the opposite sides of each column move in opposite directions? But there is a deeper difficulty. We have no basis for supposing that the imaginary path intersects the oblique at any particular point. All the sensitive points on one side of the retina are sweeping over the oblique and are cutting it at all conceivable places. Therefore, if for convenience we choose to mention a special point of intersection, we have no right to select one that is favorable to the result desired in preference to one that is absolutely unfavorable. For example, in our supposed case, we are in no way warranted in placing the point of intersection at the left end of the oblique rather than at the unfavorable right end. Under the former circumstances the angle would enlarge upwards, in the latter case downwards. In the one case we should have found an apparent explanation; in the other we should have met with a blank contradiction. The truth is that Helmholtz's theory is simply inapplicable. And may it not be that his silence in reference to the cause of particular directions of the illusory motions is not without important significance?

Though the evidence now adduced in favor of the purely retinal hypothesis may be regarded as sufficiently conclusive, one or two further points may give added weight.

The illusion does not always appear instantaneously when the eyes begin to move. A certain degree of retinal fatigue seems requisite for the full intensity of the phenomenon. This may be most effectively obtained by a moment's steady fixation of the point before beginning the movement of the eyes. This secures a slight after-image and provides a retinal point of reference for the ensuing movements of the various stimulations, and the illusion attains its most lively form.

At the same time a good illumination is a prime necessity. In a dim light the movement of the fixated point must be much slower than in a strong light, and the illusory movements are thereby greatly reduced in vivacity. Or if the movement of the eyes be the same for the two conditions of illumination, the illusion will almost completely vanish with a degree of illumination that is still sufficient for the clear perception and distinct discrimination of the lines when the eyes are at rest. Thus we find that the most advantageous conditions for the illusion itself are precisely those which are conducive to the most marked sensational effects.

A further point of interest is the heightening effect of *contrasting* illusory movements simultaneously present. In the Zöllner pattern these contrasts are already provided for, since alternate lines are moving in opposite directions. An isolated column, however, whether taken from the 'pattern' or constructed without the central vertical, gives an illusion of greatly diminished liveliness. Consequently in the experiments above described it is much more satisfactory to have a column of oppositely sloping obliques—most conveniently taken from the 'pattern'—ranged alongside of the particular column under investigation.

A peculiarity about these contrasting effects is that in the majority of cases only one column seems to move. This one column absorbs, as it were, and appropriates to itself whatever motion the other may have in the opposite direction. In my own case it is the nearer column which appears to stand still. This seems

to discharge its functions by acting as a reference column. As such, however, it undoubtedly contributes strikingly both to the clearness and to the apparent excursion of the illusory motion of its companion. This effect of contrast is prettily demonstrated by taking any pair of adjacent columns from the 'pattern' and then comparing the vividness of the illusion on that column which exhibits movement with the relatively feeble illusion resulting when the 'quiet' member of the pair is replaced by a column of short horizontal lines. The latter column might indeed be supposed to furnish a basis of reference, but its lack of illusory movement makes it practically without effect upon its companion.

It is, presumably, this principle of contrast which must be appealed to in explanation of certain minor phenomena to be observed when the complete diagram is 'shaken' before the eyes. Holding this in the horizontal position and shaking with a quick, short motion of the hand, all columns fall into their appropriate movements. But if the shaking movement be somewhat longer and less rapid, only columns 2, 4 and 6¹ will present the illusion. Now and then, possibly, the other columns will momentarily take on motion, but in general they seem to play the reinforcing rôle. That columns 2, 4 and 6 should display the illusion seems to depend upon the fact that they are most advantageously situated within the set of columns for receiving the full influence of contrast. That it does not depend upon the kind of the column, that is, upon the sort of slope that the obliques of 2, 4 and 6 happen to have, is shown by the fact that if a diagram be constructed by replacing each column by its opposite, the illusion will still be confined—at least most vividly—to the columns occupying the same positions as 2, 4 and 6.

It remains now to examine those widely accepted elementary phenomena upon which Helmholtz principally based his explanation. I refer to those oft-quoted cases, which the accompanying figure, Fig. 4, will call to mind immediately, where a direction-contrast, developing under the very eyes, graphically

¹ See Fig. 1.

performs its work of producing the overestimation of an acute angle.

Helmholtz's claim was, as every one knows, that, if the end of one leg of a pair of compasses be followed by the eye as it moves over the path CD , across the line AB , the two halves of the line will appear respectively to assume the positions indicated by the lines aa' and bb' . That is, the path of the com-

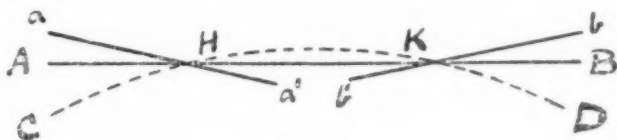


FIG. 4.

pass-point has made acute angles with the intersected line, these have been perceptually enlarged, and the left and right halves of AB have been seemingly forced out of position in the process of accommodating themselves to this unavoidable and insurmountable illusion of space perception. At first sight the phenomenon appears to be the isolated element of the illusory movements that we have been discussing above. The path of the compass-point as it cuts the line is the representative of the many paths swept out by the retinal elements as they intersect the obliques of the Zöllner pattern. And as either half of AB in Fig. 4 swings about the point of intersection, so the intersected obliques of the 'pattern' swing about and give the appearance of vertical motion to the columns. We have already seen plenty of reasons for rejecting this interpretation as applied to the larger phenomenon. And if now we are able to find some other interpretation for this supposed elementary illusion, all possible grounds for clinging to the explanation of Helmholtz will have been taken away. That an illusion of direction is to be seen when the conditions of Fig. 4 are fulfilled is unmistakable. But a really complete description of what happens seems never to have been given. For the more convenient study of the matter a heavy line of 4 mm. thickness and 50 mm. length was drawn upon paper and the latter fastened to the wall directly in front of the observer. A slender rod 475 mm. long and tipped with a steel knitting needle swung from a center be-

low in such a way that the tip of the needle traced an arc 22 mm. above the line at the highest point. This arrangement reproduces all the essentials of the original, while allowing greater facility in the way of control and observation.

What happens to the line *AB* as the point is carefully followed in its course is certainly not adequately represented by the usual oblique straight lines of Fig. 4. There is rather an apparent *sagging* of the line at the center, somewhat as represented in Fig. 5. And furthermore this whole appearance of



FIG. 5.

distortion may be produced by confining the moving point to the portion of the arc that lies between *H* and *K* (Fig. 4). That is, the actual intersection of the line by the path of the moving point is not at all essential to the illusion. This in itself is sufficient to cast an interesting suspicion on the hypothesis of direction-contrasts. For here we are obviously dealing with the path of the fixation-point, and not with ideal lines traced by any point whatever of the retina. A careful examination of the behavior of the line during the movement of the eye from *H* to *K* will, I think, reveal the following: If the moving point stop short of the crest of the curve, the line *AB* seems simply to move downwards, keeping always parallel to its original position. This is a necessary consequence of the eye's movement obliquely upwards, for the perceptual effect, or at least the retinal effect, is precisely what it would be were the line to move downwards and to the left. The sidewise component of the movement is not noticed unless the line has some conspicuous marks upon it. Again, if the moving point pass from a position just beyond the crest of the arc down across the line, the latter will seem simply to move upwards. In neither of these cases is any departure of the line from its original direction perceivable. But if now the moving point be followed throughout its entire course, or even if it be confined to the position *HK*, the sagging of the line appears. But this appearance is manifestly only a

necessary perceptual interpretation of three contradictory perceptions that come about successively as the point moves. First, while the eye is approaching the summit of the arc the line moves down, then as the summit is being passed it ceases altogether to move, and finally as the eye sweeps downwards the line moves upwards. That is, during the passage of the eye over the length of the curve, the line has presented three successive phases—upward movement, no movement, downward movement. Now bearing in mind that the attention is most sharply given to that portion of the line that is at any moment just below the fixation-point, and considering further that the final perception of the whole line must be made up of these three partial perceptions which follow each other so rapidly as to fuse into a single resultant, the illusory sagging line seems the only possible spatial form that could be perceived. The illusory sagging appears just because three incongruous perceptions are forced into one by the fact that the line *AB* must be perceived as a continuum, and the three modes of behavior of its three different parts must be apperceived as occurring upon an unbroken, continuous line.

There is no need of appealing to the influence of direction-contrasts here. We are merely in the presence of one of the so-called 'illusions of interpretation.' And as thus viewed this phenomenon has not the slightest connection with those other illusory movements which form the subject of this paper. Different causes are operative in the two cases, though both are reducible to relatively simple retinal happenings and need no underlying spatial peculiarities for their full and satisfactory explanation.

Helmholtz, it will be remembered, claimed to find evidence for the working of a direction-contrast when the path of a moving point is along a *straight* line—whether actual or ideal is indifferent—cutting a horizontal line at a small angle. The use of the compass-point arc in place of the straight-line path was, according to Helmholtz, simply to secure more vivid results. But I cannot refrain from thinking that Helmholtz's zeal to obtain a preconceived result led him here to fall into error. And I cannot find that others have verified the alleged observation that

the main line seems to incline away from the path of the moving fixation-point as the latter cuts across it. I have carefully examined the matter, using the same heavy line as above and letting the imaginary oblique be traced by the tip of a stiff wire fastened below to a tiny car, sliding upon an inclined plane. The moving tip was of course close to the plane of the paper bearing the line, and the angle of intersection was 5° . Now with an excursion of the moving point of about 10 cm. on each side of the line, nothing whatever beyond the usual and inevitable up-and-down movements of the line was to be seen. There was not the faintest vestige of tilting of the line. Nevertheless such tiltings were occasionally seen, yet so inconspicuous as to be easily overlooked, when the imaginary line was made as long as the horizontal itself. But this, once more, is only an illusion of perceptual interpretation. As said above in discussing the companion illusion, it is the portion of the line just above (or just below as the case may be) the moving fixation-point which seems most of all to be in movement. Accordingly when the eye, passing say from left to right, arrives at the end of its path, it is the extreme right end of the horizontal that is at that instant in most vivid apparent movement downwards. The rest of the line is still visible, of course, but it is relatively motionless. What other possible result then for perception than that the horizontal seems to be assuming a slight inclination downwards. The correctness of this view of the case is made overwhelmingly probable, it seems to me, by the fact that the illusory inclination of the line does not appear at about the time the line is being intersected, as we should expect on the theory of direction-contrast, but comes rather at the end of the whole path. Any one who will carefully repeat the experiment will be convinced, I feel sure, that the illusion here is due to the manner of interpreting motions on the retina and not to the over-estimation of angular magnitudes.

It may seem that these elementary phenomena have been dwelt upon at needless length. But I cannot help thinking that a real error has been firmly attached to them ever since Helmholtz first made his communications in respect to them. And since they have been so often referred to and so often made the

explanatory basis for other illusions which have no actual relationship with them, no amount of pains is too great which shall contribute to a more accurate understanding of their provoking cause.

SUMMARY AND CONCLUSION.

We have now the various facts and arguments before us. In conclusion these may be gathered together for convenient inspection. To begin with, we saw that no one of the current explanations for the movements to be seen on the Zöllner pattern could stand aright before certain easily verified facts connected with the illusion. That of Thiéry, in terms of equivocally perceivable prism-edges, could not meet the case where the illusion was produced by simply 'shaking' the diagram before the eyes. Grave discredit was seen to be cast upon Helmholtz's explanation by the observation that no change of inclination was to be seen on the part of the obliques during the presence of the illusion, though such change of inclination was supposed by Helmholtz to be the underlying cause of the apparent movements. And finally Judd's explanation was rendered inapplicable by the removal of the central verticals, there being thus no sides of angles to be falsely estimated.

The proposed explanation in terms of peculiarities of the movements of stimulations upon the retina was then supported by several considerations: First, that the rates of the actual and illusory movements correspond; second, that the excursion of the illusory movement depends directly upon the degree of the slope of the obliques; third, that the illusory movements, presenting, as they do, a behavior corresponding perfectly with that of the eye in respect to rest, movement and kind of movement, give the impression always of the passing of visual stimulations along the retina: and, lastly, that the particular directions in which the differently constructed columns move are now for the first time adequately accounted for.

And then finally those alleged basal phenomena in connection with single lines were seen to be entirely capable of a new interpretation, which removes them completely from any kinship with the more complex illusion which Helmholtz supposed them to explain.

Not only then have the curious movements of unrest that the Zöllner pattern may be made to show found an explanation that satisfactorily accounts for all the peculiarities connected with them, but in addition the fundamental basis for the original and most widely accepted explanation has been swept away. This new explanation, by being substituted for the old one, will probably have no far-reaching effect upon the interpretation of other geometrical optical illusions. Nevertheless it is decidedly worth while to get each individual illusion set in its true light.

ELEMENTS OF CONSCIOUS COMPLEXES.¹

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Two conceptions, variously modified, lie at the basis of psychology. It may be treated after the fashion of Höffding and Brentano, of Stout, Royce and Baldwin, as a study of the activities or the relations of conscious selves; or else, as by Münsterberg, by Titchener and for the most part by Külpe, psychology may be regarded as a study of contents-of-consciousness, of percepts, images, feelings and the like, considered without explicit reference to the selves for which they exist. One of the most serious errors of the psychological theorist is the conviction that one of these methods must be 'right' to the exclusion of the other. On the contrary, both are valid and useful though they are entirely distinct. The psychology of selves in their relations is a genuine science, lying at the basis of history, ethics and philosophy, whereas the psychology of the contents-of-consciousness facilitates a close and helpful parallel of psychic facts with physical and physiological phenomena.

Equally misleading and far more frequent are the confusion of these methods and the alternations from one to another within the limits of one system. Wundt, for instance, adds to his analysis of *Vorstellungen* an uncoordinated doctrine of 'inner activity'; Külpe treats psychology, through four hundred pages, as a study of elements and their synthesis, and then suddenly swings over to the other point of view and considers consciousness-as-a-whole and attention as its state or condition (*Zustand*); and James oscillates without explanation between the two methods of regarding consciousness, now as a 'stream' of thoughts or a succession of 'feelings,' and again as a set of 'cognitive functions' or 'operations.' The ordinary division of the spoils between these methods of psychology allots to the study of

¹ Read, in part, before the American Psychologica Association, New Haven, 1899.

psychic contents the analytic treatment of percepts, of images and sometimes of emotions, but reserves for the other method the treatment of memory, of thought and of will. The truth is, however, that this confusion of two governing conceptions within one system is as unnecessary as it is misleading. Perception, as truly as will, may be treated as a form of self-consciousness; and, on the other hand, thoughts like images may be made to disclose their elements. In other words, every conscious experience may be studied from either point of view. The purpose of the present paper is, however, the consistent treatment of all psychological material from only one of these standpoints; the demonstration that every conscious experience may be treated as a content-of-consciousness and analyzed into its constituent elements. The immediate problem is the enumeration and the grouping of these elements.

The basis of the classification which follows is the rigorous conception of a psychic element as a distinct and absolutely irreducible part of a conscious content. This definition has been already outlined by the writer in a discussion of one of its corollaries; the impossibility that a sensation, if defined as element, should have attributes.¹ The present paper may therefore enter at once upon the consideration of certain objections to the theory which it upholds. The first of these sets forth that the term 'unanalyzable' has only relative value as applied to the content-of-consciousness. Experimental methods and careful introspection, it is stated, are constantly showing that experiences which seemed absolutely simple at the outset, are really further analyzable, as when, for instance, the supposed 'sensation' of humidity is found to be a mixture of tactual and temperature experience. The argument may be met, however, by showing that the admitted relativity and uncertainty, in the case of any given analysis, do not affect the logical accuracy of the conception of 'psychic element' nor alter its methodological value as a limiting concept. Indeed, every supposed psychic element might be shown to be decomposable, without disproving the conceivability of the element as such.

The second objection lays stress upon the artificiality and

¹ *PSYCHOLOGICAL REVIEW*, VI., 506, September, 1899.

the abstractness of the psychic element. It urges that the simple and undiscriminated content-of-consciousness is always first in actual experience, that the analysis of it is always a later, reflective process and that the fact-as-analyzed is radically different from that earlier undiscriminated content. All this, again, is freely granted. The element of consciousness is indeed only a distinguishable, not a concretely separable experience; the psychic fact is primarily unanalyzed; and analysis is a reflective, and not an immediate, procedure, never to be taken after the associationist fashion, as an account of the way in which contents of consciousness have been actually built up out of separate elements. Yet psychological analysis, through attention to the remembered parts of a psychic complex, though it is neither a primitive nor a universal experience, is nevertheless a necessary, scientific method and is employed by all psychologists to one degree or another. Consistency demands therefore that the analysis be carried out as far as possible; and the irreducible element is really no more radically unlike the concrete experience than the mixture of quality, intensity and extent which ordinarily goes by the name 'sensation.' Moreover, this complete analysis has practical value as well as theoretical necessity, for it stimulates close introspection and furnishes, as will be shown, a basis for the correlation of psychic with physical phenomena.

Quite opposed to this objection to the undue simplicity of our psychic element is the teaching of Professor Münsterberg's recent paper¹ that the 'really indivisible elements of mental facts' are not sensations nor in fact any observable phenomena of consciousness, but rather absolutely dissimilar psychic atoms, which "no psychologist will ever observe in his own consciousness," since they are 'merely constructions for the purpose of explanation.' An adequate discussion of Münsterberg's argument would fall without the limits of this paper. Yet even if the psychic element—a seeming paradox—is an unobservable atom and therefore not an object of consciousness, it is nevertheless of advantage to consider what are the simplest phenomena of consciousness.

¹ PSYCHOLOGICAL REVIEW, VII., 10, January, 1900.

Assuming then that attentive introspection may discover elements of consciousness, we may next seek to formulate anew their distinguishing characteristics. The fundamental criterion of the element of consciousness is certainly this: its persistent distinctness from other elements and its unassailable simplicity. The element of consciousness is therefore the experience which regulated, repeated and verified introspection fails to analyze further or to identify with some other. This indeed is the only standard by which to estimate the element. The other criteria which we shall consider are clearly supplementary; they merely substantiate our introspective distinctions and assist us in classifying the elements which introspection has already disclosed. Our question, therefore, in its simplest form, is this: What sorts of elements do we actually distinguish?

I.

The group of psychic elements which we may first consider is the one admitted by all psychologists, the class of the sensational elements. 'Blue' and 'sour' and 'warm' and the rest, are unanalyzable yet distinguishable features of my experience and seem also entirely different from other elements, as for instance pleasantness. But besides this indispensable criterion of the elements—the observed distinctness and unanalyzableness—there are certain observable characteristics marking off the sensational elements from others. The first of these is the existence of a probable physical correlate for each sensational element. Thus, the rate of ether vibrations is the physical condition of each color; the amplitude of the vibrations is the condition of each brightness or color-intensity, the rate of atmosphere vibrations is the condition of each pitch, and so on with more or less exactness of nomenclature according to the degree of advance in the physical sciences. Sensational elements may further be distinguished from all others by the fact of their readily assignable physiological conditions and by the peripheral as well as central nature of these excitations. These marks of the sensational element are indeed so universally admitted that they do not demand detailed comment.

Reflective observation however discloses a final important

criterion. Sensational elements are present in every concrete experience, even when they are not predominant within it; and a given content-of-consciousness may conceivably consist entirely of sensations without the admixture of any other elements. A percept for instance need include no affective elements; it may be perfectly indifferent. It is this ability to stand alone, as it were, in consciousness which justifies the epithet 'substantive' as applied by James to sensations.

Within the class of sensational elements, thus marked off from others, psychological method distinguishes—under one name or another—qualities and intensities. (Extensities should be ranged under the head of qualities—if indeed they are not held to be complexes, including always motor or tactual experiences.) The question of the method of differentiating from each other these different sorts of sensational elements—the qualities and the intensities—is not altogether easy. There are of course the very clear physical and physiological distinctions. Extent of physical vibration and place of excitation condition the sensational quality, whereas amplitude of vibration and degree (with locality) of physiological excitation are the accompaniment of the sensational intensity. But another feature, a reflectively observed characteristic, distinguishes intensity from quality, by contrasting them in virtue of their possible serial arrangement.¹ Aside from what may be called the complex series in which sensational qualities may figure (color series like 'red, orange, yellow, yellow-green' or tone-series like C-CE-EG, in which the likeness of the successive terms is due to the presence of identical elements), sensational qualities are also capable of simple serial arrangement. Such series as 'red, yellow, green, blue,' or C-D-E-F are illustrations. Now the serial character of this succession is due to an increase not of the quality but of the difference. In other words the consciousness of 'more' which characterizes every step of a series attaches itself not directly to each quality but to the recognized likeness or difference of each quality as compared with its neighbors. Fully expressed, such a tone-series is not therefore C-D-E-F-G, nor yet:

¹ The theory of the series underlying the distinctions which follow will be recognized as that of William James. Cf. *Principles*, I., 489 *seq.*; 530 *seq.*

C
 D = more C
 E = more D
 still more C

but rather,

C —
 D — different from C
 E — different from D
 more different from C
 F — different from E
 more different from D
 still more different from C

Intensities, on the other hand, are capable of direct serial arrangement. The increase is of the intensity, as it were, that is, the 'feeling of more' as James calls it, is directly connected with the consciousness of 'bright' or of 'loud' and our series becomes 'bright—more bright—still more bright,' or 'loud—more loud—still more loud,' and so on.

In all these ways, and fundamentally always by their immediately observed distinctness, the sensational elements are distinguished from the others, and within this group the qualities are contrasted with the intensities.

II.

The second group of elements of consciousness includes as its most undisputed subdivision the affections of pleasantness and unpleasantness, admitted by almost every one as elemental and distinct from sensations. Besides possessing the ultimate criterion of observed simplicity and distinctness, these are differentiated from sensations in that, in their case, there is no assignable physical stimulus—no definite form of physical energy which is the invariable accompaniment of pleasure or of unpleasantness; and because there is in all likelihood no distinct bodily organ by whose functioning each is conditioned. Theories of the physiological basis of the affections do indeed differ widely, but whether one hold with Titchener¹ that they are explained by bodily anabolism and catabolism, or with Marshall² that they are due to the pressure or absence of 'stored-up energy' in any one

¹ *Op. cit.*, § 31; *Primer of Psychology*, § 24.

² *Pain, Pleasure and Æsthetics*, p. 191 *seq.*; 222 *seq.*

organ, or with Münsterberg¹ that pleasantness and unpleasantness are 'values of a sensation parallel to the local situation of the discharge' of centrifugal or outgoing cortical energy—on any one of these theories it is evident that there is no definite 'organ' of pleasantness or of unpleasantness, in the sense in which the retina is the organ of color and the taste bulbs organs of taste; so that the physiological correlate of affections is a general condition independent of any particular organ.

In two important particulars of another sort, affections are distinguished from sensational elements. They are not always present and they are never conceivably alone in consciousness. One's perceptual or reflective experience may be utterly indifferent and devoid of affective coloring; and one can not imagine bare pleasantness or unpleasantness; there is always an agreeable or a disagreeable somewhat—a pleasant familiarity, or an unpleasant sound. Sensations, on the other hand, as we have seen, may conceivably occur without other elements in an indifferent and unrelated complexity. To mark these distinctions, affections may be called 'attributive' after the fashion in which sensations have been characterized as 'substantive.'

To the class of attributive elements, thus distinguished by the absence of physical stimulation, by the 'general' character of their physiological excitation and by their intermittent and coincident occurrence, other elements than the affections may be referred. The so-called 'feeling' of realness and the conation or 'feeling of effort,' if admitted at all as psychic elements, will be classed in this group. But the simplicity of each of these experiences is challenged by many observers. The alleged conation is treated as an anticipatory image, and the feeling of realness is held to be identical with the consciousness of congruity. Our purpose, however, is attained when we have assigned to these alleged contents of consciousness a purely hypothetical place among attributive elements, and we need not undertake to consider the mooted questions of their elementary character.

We shall omit discussion of still another problem, the possible distinction, within this class, between qualities and intensi-

¹ Psychology and Life, p. 94.

ties. Two theories are at least formally possible. The opposition between quality and intensity can, perhaps, be made only in the case of sensations; yet it is, perhaps, observable in every group of conscious elements. If this last be true, as certain experiences suggest, then pleasure-intensities must, of course, be distinguished from pleasure-qualities. Introspection, however, is at this point so difficult that the limits of our discussion preclude consideration of the question.

III.

With its next step, this classification departs definitely from the beaten track of psychological system. Yet the doctrine that the analysis of contents-of-consciousness is still incomplete, when substantive and attributive elements have been distinguished, lays claim to the unequivocal support of careful introspection. In the experience, for instance, of matching some one color with another, the blueness, the grayness and the brightness are not the only elements involved. On the contrary, the consciousness of the likeness or difference of the given blue to certain others is the very essence of the 'matching' consciousness.¹ Similarly, it is evident that a recognition is not exhaustively analyzed when one has enumerated all the sensations included in the immediate percept and the associated images, the organic sensations accompanying the bodily attitude and the pleasantness of the experience.² Neither any one of these, nor the combination of them, is what we mean by the 'feeling of familiarity' which marks the memory-image and the recognized percept. The experience of 'wholeness,' which characterizes our judgments, and the 'feeling' of 'anyness' or 'generality,' which on any conceptualist theory, is the essential feature of the general notion, are further illustrations of experiences so radically different from sensations and affections that they may neither be identified with them nor reduced to them. As James declares,³ "we ought to say a feeling of

¹ Cf. H. Cornelius, *Zeitschrift f. Psych. u. Phys.*, Bd. 22, 110.

² Cf. Titchener, *Outline of Psychology*, § 70 *seq.*

³ *Principles of Psychology*, I., 245, end. It is almost unnecessary to remark that the word 'feeling' as used in this connection by Mr. James and by the writer, is a mere synonym for 'fact of consciousness' or 'psychosis,' and does not imply any admixture of affection.

and, a feeling of *if*, a feeling of *but* and a feeling of *by*, quite as readily as we say a feeling of *blue* or a feeling of *cold*."

These elements of consciousness may be described, for a reason which will later appear, as 'transitional.'¹ The proof of their existence is, as has been indicated, the introspective discovery of actual experiences which simply are not analyzable into substantive and attributive elements. We do have, for instance, conscious contents corresponding with such words as 'different,' 'whole,' 'many,' 'more.' These certainly are 'affectively toned,' but the sense of pleasantness or unpleasantness which they subtly convey does not exhaust their meaning. The only sensational elements involved are those of the mere word-as-perceived; but the associationist contradicts the plainest results of introspection when he claims² that the verbal image is the distinguishing mark of such an experience. On the contrary, we realize that if the affection and the sensational verbal elements could be stripped away from such a conscious content—if, for instance, the verbal image 'like' and the characteristic pleasure of discovering similarities could be dropped out of the 'feeling of likeness'—its center and kernel would remain untouched, as a somewhat which, in the words of James, 'feels different' from any sensation or any affection.

A comparison of this class with the others shows that, as in the case of 'attributive' elements, there are here no physical stimuli, no forms of physical energy corresponding with experiences of 'oneness,' 'connection,' 'difference' and the like. But the probable physiological conditions are different from those of the affection, as of the sensation, for they are purely intracortical. Not a definite brain-center connected with a peripheral organ, not any general physiological condition of body-as-a-whole or of any organ, and not even an outgoing cortical impulse is the physiological explanation of the 'transitional' ele-

¹ James's epithet 'transitive' has not been employed, because its basis, the duration of the physiological process, has seemed unessential and not entirely certain. It has already been made evident that this classification departs at several points from that of James. He assigns no definite place to what we have called the 'attributive' elements, unless his 'substantive feelings' are meant to include them.

² Cf. Titchener, Outline, 85.

ment, which is conditioned, rather, by some spread of cortical energy, some excitation of transverse fibers within the brain.

There is still one other standpoint from which the 'transitional' element may be compared with the others. Unlike the sensation, but like the attributive element, it perhaps is not always present in a concrete content-of-consciousness. Far less frequent than the 'indifferent' experience, yet still conceivable, is the utterly undiscriminated experience, the conscious content without observed unity, multiplicity, likeness, difference—in a word, devoid of transitional elements. From attributive, as well as from substantive, elements most transitional elements are obviously distinguished by another characteristic: they require the presence of at least two other elements. One cannot be conscious of likeness without observing the two objects which are like, or of familiarity without being conscious of something present and of something past. The only apparent exception is the feeling of 'oneness,' and of this it may be said that it certainly must have originated in connection with a consciousness of 'plurality,' which evidently requires the presence of several elements.

The attempt to enumerate these 'transitional' elements discloses serious difficulties of introspection. The experiences in question have no physical conditions and are physiologically excited by central, without peripheral, change. It is therefore alike impossible to isolate a stimulus, or to bring an element into prominence by constant variation of its accompaniments. Contents-of-consciousness which are ideally analyzable may readily defy actual analysis and may be incorrectly treated as elemental. Examples of these delusively simple experiences are the feelings of 'wholeness' and of 'familiarity,' already named. Simple as they seem, they certainly are not strictly elemental. The first named may be reduced to a consciousness of one-as-connection-of-many and thus involves at least three perfectly distinct elements, and the 'feeling of familiarity' is even more complex, presupposing the consciousness of 'sameness' and of 'pastness,' each of which in turn is still further reducible.

Bearing in mind the admitted difficulty of introspective

analysis, the following enumeration of transitional elements is expressly offered as tentative and open to correction. The 'feelings' of 'one-ness' and of 'many-ness' or plurality are fundamental elements of this class, that is, they lie at the basis of most complexes. The 'feelings' of connection, of opposition, of likeness, of difference, of 'more-ness' and of 'less-ness' are probably also transitional elements, but this list may not be exhaustive and, on the other hand, it perhaps includes analyzable contents of consciousness. The 'feelings' of 'wholeness,' of 'necessary connection' and of 'sameness' are very simple combinations of these elements, readily mistaken, as we have seen, for unanalyzable experiences; and the 'feelings' of 'familiarity' and 'generality' or 'any-ness' are still more complex contents-of-consciousness, but reducible to transitional elements.

This doctrine of transitional elements must of course contend against objections of two sorts. On the one hand, those who regard psychology as a study of functions of the self will cry out loudly against the peculiar sacrilege of reducing to mere conscious elements, to mind-stuff, as it were, the relating activities of Self or Mind. From the opposite camp, the upholders of the theory of psychology as mere study of psychic contents will come forth to do battle against a theory which, they say, admits Kantian categories, under the guise of 'transitional elements,' into the ranks of psychological facts.

To critics of the Intellectualist school the following reply must be made: The objection, if urged only at this point of our analysis, comes too late. If the right to analyze conscious contents into sensations, or into sensations and affections, has been admitted, as it practically always is, then consistency requires a continuation of the analysis until it is recognized as complete. It is unreasonable to say in effect: "While you are treating of perceptual and even of emotional experience, you may regard consciousness as a succession of ideas and of feelings and may analyze these into their elements, but when you come upon a judgment or a memory and are conscious of other than sensational and affective experience, you must change your point of view and drop your conception of elements and

talk only of the activities of Self or Mind." The method is valid throughout or it is absolutely invalid. Either it is incorrect, and not merely useless, to treat of percepts and emotions as combinations of sensational and affective elements, or it is also necessary, if introspection discloses unanalyzable contents like one-ness, connection and difference, to admit their elemental character.

The other difficulty is met by denying its assumption. Our transitional elements are not reflectively observed and classified 'categories,' they are experiences as immediate as the sensations or the affections themselves. The proof of their actual existence is the failure of all attempts to reduce memories and 'thoughts' to merely sensational and affective elements.

A very simple classification of conscious complexes may be based upon this distinction of psychic elements. Percepts and images, complexes in which sensational elements predominate, may be contrasted with feelings and emotions, complexes in which affections are the essential feature; and both may be distinguished from those conscious complexes which are characterized by the presence of transitional elements, or by simple combinations of them. These are: the judgment, whose essential feature is the 'feeling' of wholeness; the general notion characterized by the 'feeling' of generality or 'any-ness'; and the memory image, in which the 'feeling' of familiarity is of paramount importance. All this is suggested by the following tabulation which summarizes the most important conclusions of the discussion:

A. ELEMENTS OF CONSCIOUSNESS.

Criterion: Distinctness and Unanalyzableness.

I. 'Substantive' or Sensational Elements.

- | | |
|------------|--|
| Criteria { | 1 (Psychological) Conceivable presence of elements of another order. |
| | 2. (Physiological) Definite peripheral and central excitation. |
| | 3. (Physical) Definite form of physical stimulation. |

a. Sensational 'qualities.'

- | | |
|------------|---|
| Criteria { | 1. Not directly accompanied by 'feeling' of 'more' or 'less.' |
| | 2. Varying with locality of physiological excitation. |
| | 3. Varying with rate of physical vibration. |

b. Sensational 'intensities.'

- Criteria { 1. Directly accompanied by 'feeling' of 'more,' etc.
 2. Varying with *degree* (and with locality) of physiological excitation.
 3. Varying with amplitude of physical vibration.

II. 'Attributive' elements.

- Criteria { 1. Not always present and not conceivably occurring alone.
 2. Physiological excitation: 'General' bodily condition.
 3. No characteristic physical stimulator.

a. Affections.

? b. 'Feeling of realness.'

?? c. Conation.

III. 'Transitional' elements.

- Criteria { 1. (a) Not always present and not conceivably occurring alone.
 (b) Requiring, each, the presence of at least two other elements.
 2. Physiological excitation: intra-cortical.
 3. No characteristic physical stimulation.

B. COMPLEX CONTENTS OF CONSCIOUSNESS.

I. 'Substantive' (Sensational) experience predominates: { Percept
Image

II. 'Attributive' experience predominates.

a. Affections:

Emotions, etc.

b. 'Feeling of realness':

{ The Belief
The Volition

etc.

III. 'Transitional' experience predominates.

a. 'Feeling of wholeness':

The Judgment

b. 'Feeling of familiarity':

{ The Recognized
The Remembered

c. 'Feeling of generality':

The General Notion

DISCUSSION AND REPORTS.

THE FUNCTIONAL SIGNIFICANCE OF THE TERMS 'SENSORY' AND 'MOTOR.'

In another place¹ the writer has called attention to the existing ambiguity in the use of the terms 'sensory' and 'motor' in psychology. It was there pointed out, in connection with the study of sensory and motor errors, in reading and listening on the one hand and in speaking and writing on the other, that is, in connection with errors in the interpretation of the meaning of external symbols and in the expression of meaning in external symbols, that in all so-called sensory process there are motor, and in all so-called motor process there are sensory factors. Much confusion in the study of the psychology of speech, it was there remarked, has arisen from a false antithesis of the 'sensory' and 'motor' processes. And it was there maintained that the terms 'sensory' and 'motor' in psychology, properly used, are not content, but functional distinctions. It is the aim of the present article to aid in an understanding of the true functional significance of these terms. The subject has already been treated from somewhat different standpoints by two other writers, J. J. Biervliet² and J. R. Angell.³ References will be made below to their discussions of the subject.

The words 'sensory' and 'motor' were first used in biology, in a purely descriptive sense. Charles Bell used the terms to characterize the afferent and efferent nerve bundles. This use became current in anatomical description, and found its way ultimately into existing psychological treatises through the channel of physiological psychology, which insisted on descriptions of the nervous system as preliminary to the treatment of psychology proper. Along with the

¹ A Study of Lapses, Monogr. Supplem. to the PSY. REV., Vol. III., No. 4, pp. 16 f., and 56 f.

² Images Sensitives et Images Motrices, *Revue Philosophique*, Vol. XLIV., pp. 113-128.

³ J. R. Angell and A. W. Moore, Reaction-Time ; A Study in Attention and Habit, PSY. REV., Vol. III., No. 3, May, 1896, p. 254 f. Also, J. R. Angell, in a Review of S. N. Patten's 'Development of English Thought,' *Am. Jour. of Sociol.*, Vol. III., No. 6, pp. 824 f.

genetic and functional movement in psychology greater attention has been paid to the physiological, and especially to the neurological corollaries of psychical process. As a natural consequence, the terminology of biology has invaded psychology, for the most part with beneficial results, but sometimes producing a certain amount of confusion as the result of the mixing of two alien nomenclatures. It is beyond dispute that the application to psychology of the methods of natural science has been most fruitful, but the new terminology which has accompanied this change has not been assimilated, in some instances, and then stands as an unnecessary barrier to a more intimate interaction of the two sciences. The terms 'sensory' and 'motor' are a case in point.

It is important to have a clear statement of the principle underlying the proper use of these terms because on this distinction hinges the entire significance of certain recent or pending controversies. Among these may be named the controversy over the so-called 'innervation' sensations, the controversy between the intellectualists and voluntarists, the controversy between the representatives of the bipartite and tripartite theories of the fundamental modes of consciousness, the controversy between those who call themselves the structuralists and functionalists in psychology, and the various controversies which have taken place and are taking place over the interpretation of the facts of aphasia. It is not our purpose in this place to show how this ambiguity runs through all these controversies. We shall attempt rather to show where the fundamental difficulty lies, leaving this detailed application for another time and place.

There is no quarrel with the purely descriptive use of the terms. It is when they come to be given an explanatory or interpretative significance that the possibility of error comes in. On this side, we select two typical illustrations of the ambiguity. These are found (1) in the current theories of aphasia, and (2) in the customary characterization of the kinaesthetic sensations as 'motor.' We choose these two uses of the terms for criticism because here the ambiguity stands out most obviously.

The ambiguity of calling certain forms of aphasia 'sensory' and others 'motor' becomes apparent if one attempts to gain a coherent notion of aphasia from the current literature on the subject. This ambiguity grows partly out of false theories of cerebral localization and partly out of an imperfect psychological analysis. Flechsig has shown us that the centers in the so-called sensory region of the cortex are no more sensory than motor, that, in fact, they are sensori-

motor. An understanding of this point clears up the ambiguity arising from the anatomical side. On the other hand, a more mature psychological analysis of the facts of aphasia shows that the imagery involved in the so-called 'motor' types of aphasia is as much sensory as the imagery involved in the so-called 'sensory' types of aphasia. Aphasia is either all motor or it is all sensory; it cannot, from the same point of view, correctly be described as part sensory and part motor.¹

The other illustration of the ambiguous use of the terms 'sensory' and 'motor' is found in the customary characterization of the kinæsthetic imagery as 'motor.'² These are the sensations which are connected with the movement of muscles, tendons and joints. The ambiguity lies in the tendency of various writers to regard the kinæsthetic sensations as motor in some sense in which other sensations are not. This is done, apparently, in an uncritical way and for no other reason than that the kinæsthetic sensations are connected with the motor organs. That imagery is called motor which is connected with the use of these organs of expression, that is, which comes from the sense organs located in the muscles, tendons and joints. The additional idea is frequently connected with this, that the kinæsthetic imagery is in a peculiar sense motor because it most immediately serves as motor cue, that is, it is the kinæsthetic imagery which immediately precedes the centrifugal discharge which sets free the overt movement. But on the one hand, as we shall see, the fact that certain sensations are connected with the activity of organs of expression (muscles, etc.) entitles them in no way to the peculiar characterization of 'motor,' and, on the other hand, if the kinæsthetic imagery is motor because it is essential to the motor response, then all the other imagery must be motor as well, since all sensations and imagery have this same dynamogenic tendency.

In order to unify the critical remarks which have just been made and to show upon what they are based, it may be well to summarize what we conceive to be the functional unit of interpretation in both physiology and psychology. We may take as the point of departure for our constructive statement Professor Dewey's critique of the concept of the reflex arc.³ His substitute for the old idea of the reflex

¹ Collins in his 'Faculty of Speech' (1898) recognizes the ambiguity here, but he has not the courage of his convictions. Cf. pp. 4, 55, 57, 60, 160 f.

² Cf., for example, G. F. Stout, *Manual of Psychology*, 1899, p. 190 f, and 464 f.

³ *PSYCHOLOGICAL REVIEW*, July, 1896, III., No. 4, pp. 357-370.

arc is the concept of the organic circuit. By this is meant that entire process of coördination, adaptation, adjustment, to use the physiological terms, or mediation, reconstruction, reorganization, to use the psychological terms, which includes within it as functions or part-processes both the stimulation and the response, the excitation and reaction. The process of stimulation includes all the stimuli which factor in securing a response; both internal and external, organic and extra-organic. The truth is, according to this conception, there is no such thing as an extra-organic stimulus. In order to be a stimulus at all, an excitation must already have become a function within the total process of coördination. What has been said regarding the stimulus may also be said concerning the response. It has meaning, it has existence, only within the act of coördination. The mistake usually made in connection with the response is similar to that made in the case of the stimulus. It is conceived as external and overt only. The truth is that the so-called central process to which, exclusively, the *idea* is supposed to correspond is itself the beginning of the *act* of the response. The problem of adjustment is not only the problem of the adjustment of the organism as a whole to an external environment; it is also, and very largely, the problem of the adjustment of the various organs to each other within the organism. That is, the two functions of an organism, stimulation and reaction, suppose each other. We never know that an organism has been stimulated, in the first instance, in fact we do not even know that it is an organism, except through the reaction. The organism is not in certain cases expressive and in certain other cases receptive, but it is active always. Its passive or receptive (sensory) and its active or expressive (motor) aspects can be separated only for convenience of analysis, and even then such a division is of doubtful value. Useful lines of cleavage cut right across such a distinction.

From this point of view the conception of an arc clearly is inadequate. The arc might just as well be a straight line so far as the theory is concerned, though those who hold to this view invariably, though inconsistently, bring the two ends of the arc together in some way. In every organic circuit it is as true that the responsive attitude through the kinæsthetic imagery controls the selection of the stimulus as that the stimulation determines the character of the response. The whole nervous system is a mechanism for such interaction. This is made possible through the accommodatory apparatus of the special sense organs, the muscles concerned in the adaptation of the organ to the external excitation giving rise to the kinæsthetic imagery which is di-

rectly correlated with that received from the motor organs concerned in the response. The only difference is that in the case of the special sense organs the overt motor function of expression has been reduced to the minimum in the development of the race, while the dominant imagery connected therewith has absorbed the whole field of consciousness. The kinæsthetic sensations, though they arise chiefly in connection with the motor organs or muscles, are not for that reason any more motor than sensations from any other part of the body. The only difference is that in the case of the special sense organs the parts are adapted for the reception and transmission of impulses centripetally and the motor parts connected therewith are overshadowed in this development or modified into a special accommodatory apparatus for this same end, while in the muscles the chief function which has developed is that of the transmission centrifugally of the nervous impulses and their mediation into expressive acts while the receptive and centripetal process is greatly overshadowed, being represented in the sense organs of the muscles, tendons and articular surfaces.

A lucid statement of this interaction of stimulus and response has been made by Professor Angell in a paper on 'Reaction-Time' in the *PSYCHOLOGICAL REVIEW*.¹ Suppose, he says, that we attempt to analyze any typical coördination. Take the case of the response to a signal as simplified in the reaction experiment. Let it be an auditory-hand reaction. Then the process of adjustment or the coördination may be stated in terms of habit and attention as follows: These are Professor Angell's words: "As the reagent receives his instructions for the reaction, he formulates in imagination what he is going to do. This formulation, the getting in mind what he is to do, is his attention to the act. Whatever may be the detail of imagery involved in this formulation, it involves primarily the coördination of two groups of incoming sensations, one from the ear, the other from the hand, started by the operator's descriptions. From this, two distinctions may be drawn: (1) As related to the act of attention, these two sensation groups are its stimuli; and each group is as much stimulus as the other—the sensations from the hand as much as those from the ear. The 'reaction' as meaning the whole act to be performed is not the mere response of the hand to the ear, but the act of attention in coördinating the incoming stimuli from *both* the hand and the ear. Concerning the 'sensory-motor' distinction it follows that, since the stimulus, *i. e.*, the material for the act, lies in these incoming currents from both hand and ear, as related to the whole act, both

¹ May, 1896, III., No. 3, pp. 254-258.

'forms' may be regarded as equally 'sensory' or equally 'motor.'

(2) In relation to each other *inside* the act of attention, most discussions of the subject appear to make the ear process merely a stimulus to which the hand adjustment is merely a response. But the question arises, What holds the ear to its work? Why does the reagent maintain his listening attitude? It may be replied that it is 'because he is told to.' But he is not told to listen any more than he is told to move his hand. If the telling suffices in one case it should in the other. Moreover, he is not *merely* to listen, or even to listen just for the click, but to listen for the click as a *pressure signal*. The hand therefore is stimulus as well as response to the ear, and the latter is response as well as stimulus to the hand. Each is both stimulus and response to the other. The distinction of stimulus and response is therefore not one of *content*, the stimulus being identified with the ear, the response with the hand, but one of *function*, and both offices belong equally to each organ. The reason the movement of the hand is so often treated as the *mere* response to the ear as its *mere* stimulus appears to be that the whole act, or 'reaction,' is identified with the movement of the hand. But the entire act is the act of attention in coördinating the two groups of stimuli coming from both hand and ear."

This point of view enables us to understand the significance of Flechsig's contention that the so-called sensory cortical centers are as much motor as they are sensory. It is not necessary to give here an exposition of Flechsig's argument. Statements of it may be found in his 'Gehirn und Seele.'¹ The older view looked upon certain areas as sensory and upon certain others as motor. The error of this view from the physiological and psychological standpoint lies in supposing that the sensory phase of the process which answers to a given coördination corresponds exclusively to impulses transmitted from the special sense organs.

This standpoint gives us also a fundamental basis from which to criticise current theories and classifications of types of aphasia. The old idea that certain forms of aphasia are sensory and others motor was due largely to an inadequate conception of the nature of the organic circuit. There was set up a false antithesis of the sensory and motor processes. The 'moteur' was distinguished from the 'auditif' and 'visuel' by reason of the fact that he had a consciousness of the beginnings of the movements that are necessary in order to express the

¹ P. 65; statements in English will be found in Collins, Faculty of Speech, p. 106 f., and in an article by Barker, *Jour. of Nerv. & Ment. Diseases*, 1897, p. 336 f.: Cf. also Biervliet, *Revue Philosophique*, 1897, p. 122 f.

idea in external form. It was not seen that this so-called motor or kinæsthetic imagery is necessary to the auditory or visual processes as well. It was not seen that it would not be possible for the individual to understand or interpret any sensory process of stimulation if there were not at least an incipient motor discharge. So that the distinction which so many writers on aphasia have made between motor and sensory aphasia as involving disturbance in the expression and in the reception of speech ideas, respectively, however practical it may have been from the clinical standpoint, is certainly not a sound one psychologically.

Aphasia should be classified either entirely from the standpoint of its sensory or entirely from the standpoint of its motor aspect. The two categories should not be mixed. If classified from the sensory standpoint the classification is a psychological one. If classified from the motor standpoint the classification is a physiological one. Of course, in a complete study of the subject, aphasia will be treated from both sides; from the psychological side when we speak of the types of imagery involved, and from the physiological side when we speak of the reactions which are the external expression of this imagery. But in classification for the sake of clearness these two aspects must be kept distinct. If a classification were to be made from the side of the sensations or imagery involved, we should have three main types of aphasia: auditory, visual and kinæsthetic. If a classification were to be made from the side of the reactions involved, we should have two chief forms of aphasia: aphasia proper (aphemia) and agraphia.

The erroneous use of the term 'motor' has led to the neglect of the proper sensory aspect of the kinæsthetic sensations. Biervliet has called attention to this in the paper to which reference has been made.¹ He employs the following illustrations. A child hears and sees us speak. This produces auditory and visual sensations in the child, and these sensations in turn arouse kinæsthetic images either because of an inherited mechanism with paths of least resistance connecting with the kinæsthetic area or because of the associations of the kinæsthetic imagery with similar motor reactions in the past experience of the child. By one or both of these means the child comes to get a kinæsthetic as well as an auditory and visual image of the world. Then the child endeavors to react in a manner which will produce in himself sensations which will correspond to those derived from hearing other persons speak. Here, plainly, the kinæsthetic imagery may be viewed either as sensory or as motor according to whether you view it

¹ *Op. cit.*, 3, 117 f.

from the standpoint of the process of stimulation, the speaking of the other person, or from the standpoint of the process of response, the so-called imitative reaction of the child. Or, take the audience in a theatre while a thrilling situation is being portrayed. Many persons who are not accustomed to self control will show the dynamogenic tendency of the visual and auditory imagery in the altered tonicity of the muscles as revealed by the half-suppressed reflexes which escape them. The kinæsthetic imagery which is involved here may be regarded from either of the two points of view. It may be viewed as imagery aroused by the sense presentation (chiefly in visual and auditory terms), the performance on the stage. That is, it may be viewed as the terminus or consummation of a sensory process. Or it may be viewed as initiating these motor reactions which involuntarily escape the auditor and spectator. Here it is regarded as the commencement of the response. In other words, there is no more reason for regarding the sensations connected with the functioning of the sense organs as sensory than those connected with the functioning of the muscles. On the other hand, the sensations arising from the muscles are no more motor than the sensations arising from the sense organs. The term 'motor' here is really irrelevant. To repeat, the terms 'sensory' and 'motor' properly used are not content but functional distinctions. By this it is meant that the motor processes of the reaction or response have qualitatively no different representation in consciousness from that of the sensory processes. There is no such thing as a motor consciousness distinct from and yet on a par with the ideational and feeling consciousness. All consciousness is motor in one aspect, and all consciousness is sensory in another aspect.

This difference of aspects, when it affects the standpoint and method of science, marks the difference between physiology and psychology. The 'motor' process is a limiting concept in psychology, and the 'sensory' process is a limiting concept in physiology. The motor function is to the psychical attitude in psychology what the categorical function is to the development of the judgment in logic. It marks the limit, the end, of the psychical as psychical. On the other hand, the 'motor' process of function is the working principle of physiology. All study of function in the physiological laboratory is a study of physical expressive reactions. And all determination, all measurements of the process of stimulation is in terms of our possible reactions to such stimuli, whether the unit be a micromillimeter or a pace or a day's journey. That is, here the sensory function is the limiting concept. A given organic circuit is either all sensory or all

motor. It is not part of it sensory, say from the sense organ to some cortical center, and part of it motor, from this center to the peripheral musculature. The whole circuit is sensory when viewed in its function or aspect of mediating a conscious experience. It is all motor when viewed in its function or aspect of mediating an act. That is, from the standpoint of the experience as introspectively felt or sensed by the individual, the whole process is sensory. This is the strictly psychological viewpoint. From the standpoint of the reaction to which this experience leads, a reaction which takes place in the world of space and time and is palpable to other individuals and hence what we call objective or physical, the whole process is motor. This is the strictly physiological viewpoint.

The element of truth in the characterization of the kinæsthetic imagery as 'motor' is this, that the kinæsthetic imagery is in a peculiar sense the fundamental imagery of meaning. It is an accepted fact that the tactile sense is genetically prior to the other senses. Now the kinæsthetic sense is closely related to the tactile sense in origin. This means that, primitively, all other imagery is translated into terms of contact values. This has sometimes been called the 'practical' origin of experience. We first began to know because and for the sake of doing. We learn by doing. Consciousness genetically follows, it does not precede the act. The first imagery to be developed is the tactile and kinæsthetic imagery, and this constitutes the bulk of that vague *mélange* which is called the empirical ego.

This is the truth in any doctrine of the primacy of the will. Those who defend the existence of a motor consciousness which stands on a par with the feeling and cognitive consciousness ordinarily have these facts in mind when they insist on attention, apperception and will as distinct faculties of the mind. They do not see that all this activity, this motor side of experience, is reported in consciousness solely in terms of feeling and sensation. They do not see that activity is the one thing which, as activity, cannot be brought into consciousness. Activity is always something physical, a play of material forces; it is never psychical. Feeling and sensation are psychical; they are the meaning or significance of that activity, of that interplay of physical energies. What various writers speak of as the ever-present activity of the mind unifying all the life of sensation and feeling is itself, psychologically described, only a congeries of sensations, the kinæsthetic sensations, which differ from others only in this, that they are the invariable accompaniment and condition of the development of all the other types of sensation.

All stimulation, whether unconscious or conscious, sets up in the organism a dynamogenic tendency which can be registered by means of the dynamometer. The way in which the kinæsthetic image tends toward movement does not differ from the manner in which any image tends toward movement. The only difference is in the immediateness with which the various types of image lead to the movement. Every auditory and every visual sensation tends to call out certain responses through the mediation of the kinæsthetic imagery. This intermediate kinæsthetic imagery does not always become conscious. It may serve as a motor cue beneath the threshold. But that it may become conscious is seen in the introspective studies of such writers as Stricker and Dodge, who have analyzed their own processes of internal speech. Vision and hearing (and the same is true, though not so obvious, of smell and taste) are dependent upon the kinæsthetic imagery in a sense in which the kinæsthetic imagery as a whole is not dependent in turn upon them. The kinæsthetic imagery always stands most immediately for the movement because it arises from the incipient state of activity of the organs in which the movement subsequently is to take place in overt form. This is why the imagery rightly is called the fundamental imagery of meaning, since the meaning of a coördination does not come out fully until after the response has been made (at least in idea). With advancing civilization and culture the tactile and kinæsthetic imagery is inhibited, is kept below the threshold, that is, is brought under control by being made automatic, while the dominant imagery of conscious experience comes to be the visual and auditory. The chief difference between the thought of the man on the sense plane and the thought of philosophical reflection is that in the one case the symbols used are in terms of kinæsthetic sensations (the language of gesture, exclamation and interjection), while in the other case the symbols are to a greater degree in terms of visual and auditory sensations. That is, the kinæsthetic imagery is an operative factor in the first case as it is not consciously in the second case. Where it is not possible for the visual and auditory imagery thus to supplant the kinæsthetic, as, for example, in the case of the deaf-blind such as Laura Bridgman or Helen Kellar, the kinæsthetic imagery retains its original character and becomes not merely the fundamental, but the only imagery of meaning. In other words, it is possible to have a pure 'moteur' but not a pure 'visual' or 'auditif.'

To sum up, these considerations certainly bring out the arbitrary nature of the customary distinctions between the 'sensory' and 'motor' aspects of the organic circuit. This distinction, as ordinarily employed,

furnishes us with no psychological differentia at all, because the attempt is made to make the distinction one of content rather than of function. But viewed simply as functional phases of the process of adjustment or coördination, the distinction has a positive value. It is preferable to substitute the term 'kinæsthetic' for the term 'motor' wherever this antithesis is not intended by the use of the latter word.

H. HEATH BAWDEN.

DR. MEYER'S 'ELEMENTS OF A PSYCHOLOGICAL THEORY OF MELODY.'¹

The search for the basis of music is centuries old; it antedates the search for the philosopher's stone, the Holy Grail, the North Pole; and it is not likely to be given up by the human mind until success is attained, or a reason is found for believing the search to be in vain.

The multitude of books that purport to give 'scientific laws' of music the author declares to be unsound and dogmatic; so the field being clear in the paper whose title is given in the heading Dr. Max Meyer brings out his carefully reasoned new theory; and if he seems to be dogmatic in manner we must remember that this is customary among writers on musical subjects. While the title calls for a psychological theory, the greater part of the paper covers ground belonging equally to the physicist; and I shall review it from the standpoint of the sister science.

I.

The first word to be spoken is one of cordial welcome to one who has shown himself to be an industrious worker, and willing to leave the beaten tracks. For the ultimate explanation of musical phenomena and development must be given by the psychologist; of course he must make large use of the materials furnished by the physicist, ethnologist and historian, but he must add much of his own procuring and combine all in accordance with mental laws. And the next word is one of satisfaction that the author is not ensnared by the pretty theories of the diatonic scale, which has as a matter of fact been consciously followed by only a small part of the men who have concerned themselves with music; in this opposition to the notion that the diatonic scale underlies all music he parts company with his former teacher, Professor Carl Stumpf, some at least of whose numerous writings are known to every scientific student of music.

After the introductory pages the development of the author's theory begins thus: "When we hear successively two tones the vibration rates of which are to each other as 2 : 3, or briefly speaking the tones

¹ PSYCHOLOGICAL REVIEW, May, 1900, pp. 241-273.

2 and 3, we notice something not describable which I shall call the *relationship* of these tones." "The hearer will observe something else, namely, that after hearing 2 and then 3 he wishes to go back to 2." Generally "no melody that contains 2 can end with another tone but 2." A similar *relationship* is observed between 2 (or powers of 2 including 1) and 5 and 7 and their multiples, but not between higher prime numbers, 11, 13, etc., and the lower ones. So a table is made out containing all the powers and products of these four numbers, 2, 3, 5 and 7 arranged systematically in octaves; the author says "I have found the series to suffice when continued up to 1024" and "I have found no case where higher powers of 5 and 7 are used than 5^3 and 7^3 "; with these limitations, if the products are considered as giving vibration frequencies, the resulting scale has in the octave below 1024 d. v. 29 steps. This is the 'complete musical scale,' and it was embodied in a reed organ which gave all the notes of the table from 64 to 1024 d. v.

Then the author selected a few folk-songs, and several melodies dissevered from their accompaniment, of dates running from Mozart to Wagner, transposed them to the key of C and played them on his organ. Since there are 29 notes to the octave, there is opportunity at each point to select that one of two or three notes which produces the most satisfactory impression, so "it is in most cases very easy to determine what pitch is meant by the composer." Into the discussion of the melodies we must not go, though it is interesting to see how the author attempts to explain the tendency to progress in the melody by the hearer's constant expectation of reaching 2 (*i. e.*, in the familiar nomenclature, the tonic); but this must not be reached prematurely: so on p. 267 he gives to one C the value 63 instead of 64, thus making it a comma and a quarter flatter than ordinary readers would place it. Similarly on p. 256 the unaccented passing note F is called 21 instead of $21\frac{1}{3}$ as would be the case in 'just intonation.' But it may be questioned whether it is legitimate to dissociate the melody from the composer's harmony in any of these cases and treat it as if it were based on a non-harmonic scale; while the analysis of folk-songs is free from this difficulty, there is the equally serious one, that the tune as written is often only an approximation to the actual one, being dependent on the prejudices and limitations of the man who noted it down.

II.

In discussing this theory we may notice first the theory in itself, and its presentation, then its relations to some older work. Unfortu-

nately the title is not clear, and in the paper it is not made certain whether the author means to present some elements or all the elements, or whether all melody is to be thought of, or only that of Europeans: but these distinctions are important. However these may be, the theory is based on and logically developed from the statements quoted above: it is significant that these are not put in the usual form of axioms, *i. e.*, impersonally and generically; the wording is 'we notice * * * *relationships*'; and the 'we' in the statements about experimental work appears to be the author alone. In these statements it is the man of harmonic training who speaks; for nothing I think can be more certain historically than that these *relationships* have been unrecognized by most of the men throughout the ages who have concerned themselves about music. The results of such training on students of alien music I have discussed elsewhere¹ and need not go into it here. The author's theory is logically developed as already stated, but only up to a certain point, where it is stopped because of a reason which does not rise above the personal plane; so the scale is not theoretically 'complete,' but at most only sufficient for present needs.

The description of the new organ, by which the theories are to be upset that were current when the music that is quoted was written, is so meager that one hesitates to draw any conclusions from the experiments performed on it: then too the *relationships* noticed between its sounds will almost certainly be much more marked than between the notes of voices or instruments familiar to musical composers; and a little error in tuning or difference in quality, or in the order in which the alternatives are presented to the ear (as Planck found) may determine which of two rival notes shall be chosen. And then the fact that the preferences noted appear to be those of only a single observer, whose æsthetic training and sensibility however high are not stated, prevents our accepting them as valid generally, even for people of harmonic training.

The presentation of the scale would be much clearer if the values of the notes were expressed in terms of the usual piano scale. This is easily done by using the values for the author's 2, 3, 5 and 7 in equally-tempered semitones, viz, octave, 12.00; fifth, 7.02; major third, 3.86; natural seventh, 9.69 E. S.; add these values together where the author's numbers are multiplied, casting out the twelves; *e. g.*, $63 = 7 + 3^2$ or $9.69 + 7.02 + 7.02 = 23.73 = 11.73$ E. S., which is 0.27 E. S. flatter than the octave. The comma is 0.22 E. S.

¹ *Science*, New York, Feb. 9, 1900.

III.

Now compare this new scale and the assumptions and conclusions associated with it with the work of former students. The Greek theorists were content to work with 2 and 3, their products and powers. Zarlino broadened the foundations, as the author points out, by introducing 5, and therefore making more use of 6 than the Greeks did. Poole and others have insisted on adding 7, though it upsets the diatonic scale. This question of the 7 is not one of mathematical jugglery or of musical interpretation by the hearer, but simply this: did the composer mean to use at a certain point a natural seventh (written (7), having a pitch 0.31 E. S. below Bb in the scale of C), and score the passage for an instrument that could give it (horn, violin, etc.)? Here is one authoritative answer: Gevaert, Director of the Brussels Conservatory, says in the *Annuaire* of the institution for 1883 (about p. 200), that the natural seventh is used by modern composers, but not the natural eleventh or thirteenth. But admitting this, it is questionable whether the older composers quoted by Dr. Meyer had the command of this new note. On the other hand, the argument from use, which justifies admitting (7) to the 'complete scale,' requires also the admission of many notes which the author excludes (see Gevaert's paper). Most conspicuous is the absence of the correct fourth, 4.98 E. S., *i. e.* the fifth taken downwards, and the notes of the series built on fourths, Bb, Eb, etc.; his substitutes for F are (21) 4.71 E. S., and (675) 4.78 E. S. Only the most conclusive reasons can justify this outlawing of notes which almost everywhere, except in China, have belonged to the aristocracy of the scale; yet no reasons are given.

But Dr. Meyer is not alone in thus treating the scale. About twenty years ago Dr. Ivan Zoch¹ was impressed with the fact that most Slavic folk-songs cannot be accompanied on the piano. This led him to study the collections of songs and the native instruments, especially the Tumbura, and finally to develop a scale therefrom. He says "The natural scale must lie in natural numbers" and "all scales in which there is a greater prime than 7 appear unmusical." So the basis is

1, 3, 5, 7, 9, 15, 21, 25, 27, (35), 45, (63), 75, (81, 105, 125), 135, etc. All melodies as actually sung he found could be correctly represented on a 12-tone scale with the above numbers, omitting those in parentheses, divided by the power of 2 next smaller; his scale in Dr. Meyer's notation with the musical intervals added is as follows:

¹ Carl's *Repertorium der Experimental Physik*, 18, 748-764 (1882).

2 135 9 75 5 21 45 3 25 27 7 15 2
 0 0.92 2.04 2.74 3.86 4.71 5.90 7.02 7.72 9.06 9.69 10.88 12.00 E.S.

This scale was carefully laid out on a monochord and a piano tuned therefrom. Into Dr. Zoch's classification of the shorter scales selected out of this we cannot enter. This attempt to find a scale in which the strange and powerful Slavic folk-songs could be expressed seemed so promising that I wrote to the eminent acoustician Dr. Dvorak, of Prague, about it; his reply was the disappointing one that the theory was not proved.

Underlying these and many other studies of the scale there seems often to be an assumption that the world has but one scale and one body of musical rules, though perhaps no one expresses it so boldly except in speaking of music as 'a universal language.' It may not be amiss, therefore, to clear away any misconception by some quotations. The first is from an article¹ on Wagner's methods: "His other innovation which is not even yet acceptable to all ears, is to employ the chromatic scale of twelve equal semitones as a basis for melody instead of the diatonic scale. The whole of the music of Tristan and Isolde would be impossible under the old laws. I need only quote one example and by no means an extreme one, of a passage impossible to sing or to listen to in anything but strictly equal temperament [quoting the duett, 'Blissful Dreams']. It is a constant wonder to me how singers trained upon diatonic scales can sing this and many still stranger passages with anything approaching bearable intonation." A little earlier than the date of this article Professor Donkin,² of Oxford, 'who was an accomplished musician and had a profound theoretical knowledge of the science of music,' and wrote on Greek music for Smith's Dictionary of Antiquities, said "The structure of modern music is founded on the possibility of educating the ear not merely to tolerate or ignore, but even in some degree to take pleasure in slight deviations from the perfection of the diatonic scale."

When we go to the study of Oriental scales the assumption of a single law is found to be positively contradicted. We know by what rules many scales have been formed, and the idea of *relationship*, in the sense of the author, is often if not always wanting. The principle which gives unity to the composition is something other than the harmonic *relationship* which tacitly underlies his numerical work. Even the demand for a close on the tonic, on which he lays so much stress (p. 257) is not commonly met with in Hindu music; though the final

¹ *Musical Times*, London, October, 1896, p. 652.

² *Acoustics*, Oxford, 1870, pp. 25, v.

may be C 'accidentals' (if Dr. Meyer will pardon the word for want of a better one) have often destroyed the feeling that the tune is in the key of C; and it is rare to find a Hindu tune that seems to ordinary musicians to end right.

IV.

In conclusion, I would like to suggest to any student who would join in this search for the basis (or the bases) of music that the greatest difficulty in the study is to get into intelligent sympathy with men of alien thought, and understand both what they did and their reason for doing it; for it is almost impossible to avoid coloring the facts by our own training and associations, or those of the observers on whom we must depend. This influence of association and habituation deserves investigation by psychologists. If the student would learn of non-harmonic scales, Mr. Ellis' papers¹ detailing his and Mr. Hipkins' experiments is still the best introduction; his condensation of it for the Appendix to Helmholtz is a dry skeleton. If the student would know of the mediæval scales many papers in the *Vierteljahrsschrift für Musikwissenschaft* will give him light. If he would see how pugnacious and discordant the European theorists have been, and cultivate his sense for the historic elements of the problem, he should take such a bird's eye view as is given under 'Harmoniesystem' in Mendel's *Musik. Lexicon*; and if he would learn of the scales actually rendered by European public performers and how to train his ear for observing them, he may be commended to the admirable papers in the *Vierteljahrsschrift* for 1893, by Röntgen and Dr. Planck. Of course he will soon see that in any theory of music there are two questions involved which in this brief discussion we have not discriminated as carefully as Dr. Meyer did, viz: on what principle or principles are certain notes selected to form a scale, and by what principles, physical, psychological, æsthetic, historical, etc., are these notes rearranged to form tunes.

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WASHINGTON, D. C.

¹ *Journal of The Society of Arts*, London, 1885.

PSYCHOLOGICAL LITERATURE.

Des Indes à la Planète Mars. Étude sur un cas de somnambulisme avec glossolalie. Par TH. FLOURNOY. Paris, F. Alcan; Genève, Ch. Eggimann & Cie. 1900. Large 8°. Pp. 420.

M. Flournoy, professor of psychology at the University of Geneva, presents a remarkable case of mental automatism or subconscious personality, which fully realizes the expectations aroused by the suggestive title, 'From India to the Planet Mars'; and yet, one must hasten to say, the value of the book lies in the success with which the various phases of these 'mediumistic' phenomena have been described and traced to natural and tangible starting points. While the medium and her friends are firm in the belief of spirit control as the only satisfactory explanation of the phenomena, M. Flournoy is thoroughly opposed to any such hypothesis and finds it not only unnecessary but also inconsistent with the facts and inadequate.

The story is a complicated one. The medium in the narrative goes by the name of Helen Smith. Her father was a merchant, a Hungarian by birth, and is described as an active, enterprising, matter-of-fact man and a good linguist; though quite hostile to 'spiritualistic' notions, he was gradually won over to them by his daughter's mediumship. Her mother, born in Geneva, has always been markedly predisposed to spiritualistic phenomena of all kinds, has had 'psychic experiences' of her own, and is also involved as narrator or witness in several of the less clear and less credible phenomena of Helen's mediumship. Helen herself is described as an attractive woman of about thirty years of age, intelligent and frank; she is of good physical and mental health, presents none of the recognizable stigmata of nervous instability, if we except a six-month period of general weakness, and her mediumistic tendencies; and resents strongly the imputation of being abnormal in any respect. At the age of fifteen she became an apprentice in a large business house and has earned her living as a trusted and capable employee. She has refused to allow any photograph of herself to accompany the volume, but has consented to its publication in spite of her radical disbelief in the explanations offered. During her girlhood she was given to day-dreaming, experienced

hallucinations and unusual warnings, and was as she still is of a highly sensitive, nervous and imaginative temperament. She regarded herself as a strange and unusual person, who was in a way out of place in the everyday existence about her, and she was ever ready, though often with fear and trembling, to perceive in unusual happenings the confirmations of her imaginative creations. Adolescence brought about a consummation of many of these tendencies, in the way of more positive hallucinations, momentary lapses of consciousness and sporadic instances of automatic or 'somnambule' actions. M. Flournoy regards it as likely that these excursions in an unreal world and the tendencies to automatic expression would have disappeared normally and naturally amid the sterner realities of life (for this form of coquetting with castles in Spain and seriously mystic occupation with the less humdrum world of one's imagination is not a rare trait of childhood), were it not for her introduction to the manifestations of spiritualism. These became both rain and sunshine to the tender sprouts of her subconscious fancy and developed them into a tropical luxuriance of automatic manifestations. Table turning and rapping out of messages she accomplished at once, while a slight suggestion on the part of the 'circle' induced automatic writing and clairvoyant visions. These began early in 1892 and continued without any unusual features until the spring of 1895, when, partially under the incentive of the interested and professional presence of M. Flournoy, the 'polymorphous' automatisms of Hindu and Martian blossomed forth.

Mlle. Smith, in her present séances, enters into a trance, or rather into one of several forms of trance, the general reality of which is attested by physiological changes of breathing and attitude, by the presence of abnormalities of sensibility and movement, and by marked psychologic characteristics similar in every way to those of the hypnotic trance. Her appreciation of her surroundings, her remembrance of her trance-doings on return to her normal state, vary in the several trances; several different impersonations or trance conditions may occur in the same setting, and the most remarkable phenomena seem associated with the deepest disturbance of consciousness. Her general guide or spirit-control is one 'Leopold,' who enters partially into all her automatic 'cycles' and into her daily life. She not rarely sees him, or hears his voice; he has indicated the whereabouts of hidden articles, warned her against impending disaster, prescribed remedies for the sick, and in particular directed Helen as to what she may and may not do both in ordinary worldly and in 'psychic' situations. 'Leopold' has been a great help and also something of a hindrance

to the investigations. He alone is in touch with the subconscious strata of Helen's mental storehouse, and can by suitable suggestion be made to yield information which the normal Helen is unable to give; but at crucial points he too professes ignorance, and pronounces *licet* and *non-licet* upon attempts to bring to light hidden sources of 'spirit-revealed' knowledge. Now this factotum and mentor, Leopold, is really the disembodied spirit of Joseph Balsamo, better known as Count Cagliostro, who departed this life with a somewhat shattered reputation in 1795. Leopold seems to personify Balsamo mainly when Helen passes into what M. Flournoy calls her 'royal cycle,' in which she becomes Marie Antoinette and Balsamo her '*cher sorcier*' and devoted admirer. While the unfortunate queen seems in many ways the favorite character of Mlle. Smith's automatic repertoire, and while she assumes the part with superb histrionic realism of attitude and manner; and spends entire evenings as the queen, and partakes an actual dinner which she eats with royal appetite,¹ entertaining her real, but to her transformed, guests, gracious to her favorites and queenly to all; yet this is but the spontaneous exuberance of an imaginative creation the materials for which are readily accessible to her normal self, and many details of which have been traced to an engraving accompanying Dumas' account of Balsamo. We shall, therefore, follow M. Flournoy to India and to Mars.

The Martian cycle seems to have sprouted from a chance suggestion of one of the sitters, a M. Lemâitre, that it would be interesting to know what was going on on the planet Mars, and the further elaboration of the topic after the manner of Flammarion. This notion, 'caught on the wing,' made a great impression on Mlle. Smith's subconscious automatism, and in one of her subsequent clairvoyant visions she seems to be floating away into space and the table spells out "Lemâitre, ca que tu desirais tant!", and then she arrives at her destination which the table announces to be Mars. At this same séance she also brought messages to an old lady from her dead son Alexis Mirel, who reappears in another incarnation on Mars as Esenale. Then come descriptions of Martian houses, scenery and peoples, of customs and doings, and a bit of its fauna and flora. These are all fanciful enough and are evidently designed to be as oddly different

¹ It is interesting to note that Miss Smith has no recollection upon awakening of having eaten dinner; likewise that while apparently insensible to her surroundings she nevertheless hears the 'asides' spoken by members of her company. After dinner 'Marie Antoinette' lit a cigarette, which action was commented upon as unqueenlike by her court and was never afterwards repeated.

from terrestrial conditions as may be. There is an intermediate condition in which Mlle. Smith can be induced to use pencil and brush and yet can receive by suggestion this visualized Martian scenery; in this way we have quite a collection of illustrations of things visible upon Mars. They are not particularly interesting. The landscapes and houses are rather Japanese, or vaguely oriental; the occasional specimens of a plant and animal are unusual combinations of familiar vegetable and animal qualities, not nearly so droll as those of Edward Lear. The one really remarkable feature of the Martian epos came only after a long period of incubation or subliminal preparation; this was the Martian language. In its fullest development it included the hearing of words in this strange tongue, speaking it, seeing it visualized in space and, best of all, writing it when the medium was completely entranced and personating a Martian. However none of these processes ever appeared as fluent, extensive or completely spontaneous; yet we have short but significant and consistent messages in a wholly fictitious and strange looking alphabet. Here is one of the messages reduced to Roman characters, and its French equivalent:

Astané bounié zé buzi ti di triné nâmi ni ti di umêzé sé ĩ miré bi
tarvini,

Astané cherche le moyen de te parler beaucoup et de te faire comprendre son langage.

But how is the French equivalent known? Through Leopold, who vouchsafed a talismanic word and procedure by which the entranced medium could be induced to translate. The messages do not transcend the familiar mediocrity of spiritualistic circles, but their form is certainly a marvellous example of subliminal creative imagination if we are willing with M. Flournoy to accept them as such. M. Flournoy's analysis of the language is most minute, and he pronounces it an 'infantile' production modelled closely after the French, the only language which Mlle. Smith knows. Its syntax and the arrangement of words are absolutely identical with those of French; the vocabulary is made as bizarre as may be, but it is possible in many cases to recognize the source of the invention. In brief the noteworthy point is the holding in mind of the visual signs and the phonetic equivalent of these signs, and of their combination into words, at least sufficiently to hear, see and write brief messages; the imaginary Martian setting and corroborative, 'details added to give verisimilitude to an otherwise improbable tale,' are creditable to a subordinate personality, but they do not arouse the admiration evoked by the auditory and visual memory feats.

The Hindu cycle is even more complicated, and its element of mystery remains as yet an unsolved problem. In this, Mlle. Smith appears as the daughter of an Arab Sheik, whom she leaves to become, under the name of Simandini, the eleventh wife of Prince Sivrouka Nayaca, whose present incarnation is none other than M. Flournoy himself. This Sivrouka reigned over Kanara and built in 1401 the fortress of Tchandruguiri. Other characters are a faithful servant Adél, a small ape Mitidja and a fakir Kanga who is no other than the Astané of the Martian world. Upon this foundation there is again elaborated a complex drama too intricate to be here unfolded. The first query is whence this knowledge on the part of Mlle. Smith who, as was expected, in her normal condition knows nothing of it at all. M. Flournoy consulted specialists, but all reported that there was no such person as Sivrouka known to history. What was his surprise on finding in volume one of a history of India by De Marlès published in 1828, the entire information mediumistically revealed by Helen. But the good story must again be spoiled, for it appears that De Marlès' authority is well nigh worthless and that the facts he relates are probably far from historically correct. None the less the question as to how this information reached Mlle. Smith (who normally knows nothing of De Marlès) remains a mystery; Leopold professes ignorance and he alone knows. But, as before, the most remarkable feature is the linguistic one, for sporadic utterances of 'Hindu' are taken down from the mouth of Simandini (alias Mlle. Smith). When such words as can be recognized (which by the way Leopold will not translate or write, professing ignorance of Oriental tongues) are recorded, they are found to have close resemblance to Sanskrit words. The resemblance is only partial and crude, and M. Flournoy concludes that a casual acquaintance with a Sanskrit grammar is sufficient to account for the interspersing of a few genuine words amidst much that is unrecognizable. However, no such knowledge can be traced to Mlle. Smith and every reader may form what hypothesis he chooses until the future reveals the mystery. That the personality who composes the Hindu drama and incorporates the Sanskrit words is the same as that of the composer of the Martian and the royal cycles, M. Flournoy convincingly shows; and this is the main point.

To complete the tale, something should be said of other supernatural happenings which Mlle. Smith has experienced and which M. Flournoy accepts with a rather uncritical estimate of the reliability of his evidence, which is for him second-hand. His ready acceptance of telepathy and 'telekinesis' reveals a bias in favor of these hypotheses

which seems out of harmony with the remainder of his analyses. There are some curious personal experiences of Mlle. Smith which go to show the far-reaching character and the remarkable memory of her subliminal self.

Studies of individual psychology are rarely satisfactory, whether they relate to usual or unusual persons; yet they undoubtedly have their value. The general shaping influences of events and tendencies can rarely be applied to the individual without many 'possibles' and 'probables' and 'perhaps.' The specific and particular shape which mental processes and products assume may not be a matter of pure chance, but it is the result of so many, so trivial and so complex influences that it must be treated in much the same way. And so the psychology of Mlle. Smith is most instructive when stress is laid upon the more general features of her subliminal mentality, and not upon the concrete and detailed appearances. It is idle to speculate why her Martian alphabet assumed just the form it has; it is sufficient to note its general origin. Viewed in this way, and granting the complete truthfulness of the phenomena as described, we have a truly classical instance of the psychological comprehensiveness of the automatic self in exceptional cases. M. Flournoy has thus accomplished a valuable task and has presented his material with unusual skill. The dangers of a false interpretation of this 'psychic' tale are many, and are certain to mislead many whose interest in and powers of comprehension of cases of this kind are not equally developed. It is a pity, too, that considerations of courtesy and social obligations make it impossible to fill certain interesting gaps in the narrative; yet the delineation, on the whole, is intelligible enough as it is. It seems probable that when a sufficient number of these cases have been collected, that their general nature and law-abiding character will be understood, that a rational group of associations may be clustered about the term 'mediumistic,' and that the temptation to magnify the doings of some one of this class into a bundle of miracles will gradually pass away—all of which would be welcome consummations for the progress of Psychology.

JOSEPH JASTROW.

Primitive Love and Love Stories. By HENRY T. FINCK. New York, Chas. Scribner's Sons. Pp. 851.

This work is a compilation, rather popular in method and scope, in support of the author's thesis announced in his previous work, 'Romantic Love and Personal Beauty,' as to the very late evolution of romantic love. The first half of the work is mainly given to a discus-

sion of the ingredients of love."⁷ "It has indeed an egotistic side, including the ingredients I have called individual preference, monopolism, jealousy, coyness, hyperbole, mixed moods and pride; and it is not a mere accident that these also are the seven features which may be found in sensual love too; for sensuality and selfishness are twins. But the later and more essential characteristics of romantic love are the altruistic and supersensual traits—sympathy, affection, gallantry, self-sacrifice, adoration, purity and admiration of personal beauty." The discussion of personal beauty is an onslaught on Darwin's and Westermarck's views as to sexual selection. The latter half of the book gives illustrations of primitive love among African, Australian, Polynesian, American, Hindoo, Hebrew and Greek peoples, and concludes with a chapter on the utility and future of love in which the author follows Schopenhauer. The work contains a good bibliography and index.

It hardly needs a large volume to prove that the highest, most altruistic relation of the sexes is very late, although we believe germs and occasional manifestations occur quite low down in human life, for even in the quotations made on primitive love, the authors often note that unselfish affection is not entirely unknown, but merely very rarely known. We judge that Mr. Finck would make romantic love as late as sense of the sublime, a position which even upon his own evidence is doubtful, and the evidence itself is for the most part of slight value, being the impressions of travellers and foreigners, and there being no thorough study of a single case. The work, though readable and in a measure suggestive, is much too cursory and comprehensive to have any great value, and being conceived as a personal special plea it is not carried out in the cautious, candid, scientific spirit.

HIRAM M. STANLEY.

LAKE FOREST, ILL.

VISION.

Ueber die stroboskopischen Erscheinungen. ERNST DÜRR. Philosophische Studien. Bd. XV., Hft. 4. Pp. 501-523. 1900.

The article is a report of three series of experiments which developed out of a study of intermittent stimulation and stroboscopic phenomena in collaboration with Dr. Marbe.

The first series is no more than the casual observation, by Dr. Marbe and the author, that a disk of six sectors, alternately black and white, fused at a lesser velocity than a disk with an equal number of sectors, and the same average intensity, on which appeared a double

series of black, gray and white sectors. This phenomenon is found to be entirely congruent with Marbe's theory of 'characteristic effect groups'; and is the basis for the generalization of a new determinate of the fusion of intermittent stimuli, viz, "An increase in the number of different stimuli is unfavorable to fusion."

The author assumes that the only changes effected by the introduction of gray into the black, white series were a reduced difference of intensity between the different sectors, and an increase in the number of different stimuli. The first is favorable to fusion; the hindrance must consequently be due to the second. The general delay in the recurrence of all the stimuli is overlooked entirely; though a little further experimentation would have sufficed to show that it is this, rather than a mere increase in the number, which hinders the fusion.

The second series demonstrates that fusion in stationary stroboscopic phenomena is retarded by the same conditions that retard the fusion of sectors of revolving discs, including the conditions produced by increasing the number of stimuli. The author interprets this as 'an experimental confirmation' of Marbe's general contention, that the stroboscopic phenomena, so far as they do not produce movement, may be explained completely by the laws of intermittent stimulation.

The third and most important series was aimed at discovering the cause of our inability to detect the absence of even a considerable number of phases of a stroboscopic movement. This is neither the number of phases omitted, nor the spacial extent of the omitted movements, within certain limits, but is rather the time interval of the interruption, which shows a relatively constant critical period of something over .2", so long as the eye is free to follow the movements naturally. If some constant point of the figures is fixated, the author finds, with Marbe, that a very small interruption is noticed.

The author's explanation is as follows: When the eye follows the movement, the various phases excite approximately the same parts of the retina; and the interruption of the phases can only be noticed when it is long enough for the after-image of one stimulation sensibly to diminish before the next stimulation occurs.

This conclusion is further supported by reference to the circular movement of a point of light in a dark room. So long as the eye followed the light freely, a 'critical period' of considerable duration was found, largely independent of the arc of interruption. When however a stationary bit of phosphorus was fixated, very minute arcs of interruption were noticed entirely independent of their duration.

The phenomenon has thus a simple peripheral, rather than a central

cause, as Marbe *et al.* had held; and the author believes that the stroboscopic phenomena of movement have ceased to occupy a peculiar position and may be entirely explained by the laws of intermittent retinal stimulation, and eye movements (p. 517).

Aside from the analogy of after-images of motion, etc., it must appear as a difficulty in Dürr's explanation, that the 'critical period' of interruption for a moving point of light was about one-tenth that for stroboscopic phenomena; while in general it is hardly probable that the after-image of a moderate stimulus would prevent the recognition of an interruption approximately .2" long. Furthermore the actual eye movements, which I have been able to detect in a series of simple and large figured tests have been altogether too few and irregular to warrant the importance they are given in the hypothesis. Unfortunately the author assumed rather than investigated them.

RAYMOND DODGE.

WESLEYAN UNIVERSITY.

On the Psychology and Physiology of Reading. I. By EDMUND B. HUEY. The American Journal of Psychology, XI. Pp. 283-302.

The article is a partial report, chiefly physiological, containing a most valuable contribution to our knowledge of the eye movements in reading.

The Delabarre eye-cup and a delicate recording pointer, with time-recording attachments, gave kymographic tracings, exhibiting with great distinctness the number and relative positions of reading pauses, together with the extent and velocity of the eye movements which separated them.

The most noteworthy results are the time values of the various processes. The relatively constant forward movements average 41-48 σ . The return sweeps have a much higher velocity, averaging only 52-68 σ , although the arcs of movement are at least three times as great as the forward movements. The reading pauses average 183-191 σ in those series made under satisfactory experimental conditions.

These values are unquestionably far more reliable than any yet obtained by the aid of visual methods. The reviewer doubts, however, if we can accept them without some correction. It is possible that the extra work the eye performs in moving the long pointer with short leverage may not essentially delay it. The assumption certainly demands experimental investigation.

The author's eye reaction, after practice (181.7 σ), approximates very closely my later measurements by the visual method, which were evidently not at hand when the article was written.

The concluding section reports the extent of the reading field in successive 15 σ exposures of connected discourse.

RAYMOND DODGE.

Zur Hypothese der Sehstoffe und Grundfarben. By Dr. ADOLPH STÖHR. Leipzig u. Wien: Deuticke. 1898. Pp. 103.

In the hypothesis which is brought forward in this paper the author assumes several visual substances, and, in the end-apparatus of the optic nerve, ultra-microscopic visual elements, or corpuscles, which are excited, under proper conditions, by the visual substances. It is further supposed that the direct action of light upon the visual corpuscles is already sufficient to set them into molecular vibration, and that the effect of the visual substance is simply to modify and to strengthen this vibration; the object of this duplex supposition is to combine in one a photo-physical hypothesis and a photo-chemical one, —the physical action of light on the corpuscles is common to all animals that have the power of vision, while the photo-chemical portion of the hypothesis applies only in the case of those animals which possess the visual substances. In the highest animals these substances are three in number and they give rise to the elementary color sensations; a single visual corpuscle is excited in three different ways by the three visual substances, and if by all of them at once in a resultant 'unitary' fashion. There is no difference in the function of the rods and cones as regards the quality of the sensation furnished, but the rods are more serviceable in a faint illumination and the cones in a high light.

To particularize farther, the visual elements (corpuscles, or as they are better called later, threads) consist of minute fibers each thickly strewn over its entire surface with projecting flat plates, four in a given plane, and composed of material of four different sorts, fitted to respond to four different sorts of stimulus and to furnish three primary color-sensations together with the sensation of white. [This hypothetical structure corresponds well with that lately made out by Patten in the rods and cones of certain animals.] The most characteristic part of the theory is that these little plates are set into molecular vibration in the first instance by the direct action of light (all alike in this case, and attended by the white sensation), but in the second instance this vibration is added to by the (respectively synchronous) molecular vibrations of the three photo-chemical substances, when severally produced by light of different periodicity. It is then neither the production nor the destruction of a photo-chemical substance that is effective in

exciting the nerve-terminals, though both of these processes are brought about by the action of light; but it is during its relatively brief period of existence that a given substance (in degree corresponding to the amount of it which is present) acts upon the particular color-plate with which it is in harmony. It is remarkable how many hitherto disconnected phenomena the author has been able to bring under common rubrics by means of this conception (which is not in itself by any means too far-fetched) the different varieties of color-blindness, to mention one instance, and also color-blindness and the change of hue dependent upon changes of intensity of light are brought into a single category. In fact, this theory is an intellectual achievement of wonderful ingenuity; it is for the reader a source of keen logical enjoyment to become acquainted with it. A surprisingly large number of the characteristics of the light and color manifold it has given fitting physiological representation to. The author has built up a most ingenious and complicated system of fortifications, so to speak, in which one feature after another, bastion, curtain, gorge, is so constructed as to be, not only in absolute character but also in relative interdependence, in a close and beautiful one-to-one correspondence with the vast array of experimental fact to be accounted for. It gives a norm, and one very difficult to attain to, of what ought to be undertaken by every theory of the qualitative visual sense. And the explanations given by the author are far superior, in the great majority of cases, to those which one is accustomed to in color-theory. They are explanations in the true sense of the word—viz, a physiological conception which is set up for the purpose of explaining one phenomenon is found to have either a necessary or at least a natural consequence by which it also takes account of another phenomenon. Readers of Hering are too much accustomed to hearing it said that a phenomenon has been 'explained' by his theory when it has merely been shown that it is not incompatible with it. There is need of a pair of newly defined words by which to accentuate this distinction.

But unfortunately, notwithstanding the really wonderful amount of acumen that has been expended upon it, and the success that it achieves in the correlation of widely different facts, it seems to the present reviewer that the theory fails to meet the first and fundamental psychophysical requirement of a theory—it is not made sufficiently clear that the resultant of the color-processes is in fact—in physiology—what it is stated to be, a 'unitary' process. The main thing to be explained in a color-theory is, of course, why two fundamental colors when mixed bear still the trace of their origin, while three (or four)

do not. Another objection is that the theory is not sufficiently evolutionary—it does not appear why the three plates which mediate color in the higher animals have already in the lower a distinctive structure which is unnecessary for the purpose which they serve. This is a doctrine of preformation overdone. Dr. Stöhr says that there are a dozen other physiological conceptions which could equally well be worked out as a basis for the comprehension of color-phenomena. It is to be hoped that he will try his skill at some others. Meantime any additions to the subject which may have the effect of weakening the assumed exhaustiveness of the Helmholtz and Hering doctrines are to be welcomed with enthusiasm. *Science* once had an admirable article on the desirableness of keeping the mind evenly balanced (or proportionately balanced), when certainty cannot be attained, between multiple hypotheses. Every time it is shown how much can be accomplished in the way of the correlation and the rendering comprehensible of fact by means of a happy conception, even though a known physiological basis is wanting, the greater is the incentive furnished to the anatomist to carry forward his part of the problem.

CHRISTINE LADD FRANKLIN.

BALTIMORE.

Arbeit und Rhythmus. By KARL BUECHER. Zweite vermehrte Auflage. Leipzig, Teubner, 1899, 412 S. M. 6.

The second edition of this important work contains many additions in the form of working songs from the different quarters of the globe, descriptions of primitive forms of work and many acute remarks of deep insight. In a field where speculation was at home, where the gray barbarian was measured by the ideas and standards of the Christian adult, Buecher has introduced an empirical, scientific method with pleasing, not to say astonishing results. The theories of Aristotle and of other speculative writers must give way to the wealth of material supporting the hypothesis, or shall I say the description, of Buecher in reference to the origin of music, poetry and the dance. It seems natural that a political economist should discover the very intimate relations these functions bear to work. Ferrero has ably shown the causal relationship between work and morality; Buecher extends the relationship to song, music and the dance. In the lower layers of primitive culture work, song and the dance form a trinity in unity. They were mutually complementary in the early crude economy. The enormous labor tasks of primitive men and women, devoid of nearly all the time- and labor-saving instruments which make civilization pos-

sible to-day, can hardly be understood to-day. The hollowing of a log canoe, often with crude stone implements, took so long a time that the wood began often to rot before it was finally finished. The noble savage had to work. But like the child he had an innate dislike for work. He was almost epileptic in his passionate impulsiveness. Savagery and civilization are distinguished in nothing more than in this that the one possesses persistency of economic purpose and habits of continuous labor and the other does not. The Elmira Reformatory Reports show incidentally but convincingly that crime and habits of idleness have a close causal connection. Perhaps the greatest of all pedagogical problems is the formation in the child and in society of habits of persistent industry—to evolve from a chaos of impulses an organized self-directive system of habits engaged in public service.

All those tasks of the savage which could be accomplished with an accompaniment of rhythmic movements of the limbs, and by a rise and fall or rhythm of the voice—and nearly all the tasks of the savage can be included in this list—became easier, more habitual and often a matter of play. Compare the similar phenomena of childhood. There is a like inconstancy of purpose, the same lack of patience and endurance, the same inclination to give themselves over to ever new sensations. It is a long journey from fanciful humor and caprice to stern duty, obedience and obligation. By rhythmic movement, however, habit and automatic activity are evolved. There is an economic saving in the expenditure of energy. Tone-rhythm will be an additional aid and guidance. Rhythmic work in common, especially with the aid of tone-rhythm, will make of a body of men engaged on a common piece of work one single machine. The whole process results in an increase of productiveness and betterment of quality.

Each kind of work has its own kind of music and rhythmic movement. Singing and dancing evolved out of the economic conditions of early primitive methods of work. Work, play, singing, poetry and dancing were not separate provinces, but were necessary parts of an original economic unity. The singing and poetry are the accessories of the rhythmic body movements and were necessarily adjusted to them, often suffering severely in consequence of the enforced subjection. Dancing arose as a continuation of the rhythmic play-work movements. Nowadays they are the beginnings or endings of the harvests and other such activities. The musical instruments of primitive culture are well known to be at least in the majority of cases transformed working instruments.

The work is filled with illustrations and thus appeals convincingly

to the reader. That there are other causes, especially the erotic instinct, at work in the origin of these æsthetic qualities is not denied, but at the same time is not duly emphasized. The origin of the lyric and the drama is lightly but suggestively treated. This is another contribution showing that the so-called æsthetic arts have had a reason for their being on account of their utility. Henle's essay, *Ueber die Grazie* (Anthropologische Vortraege) also bears witness to the truth of the above statements. He remarks that those movements are graceful which attain their end with the least expenditure of means.

ARTHUR ALLIN.

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Two Cases of Synæsthesia. By GUY MONTROSE WHIPPLE, A.B.
[From the Laboratory of Cornell University.] Amer. Jour. Psy.,
Vol. XI., No. 3 (April, 1900), pp. 377-404.

Mr. Whipple has found two good cases of colored hearing, in one of which the colors seem to have a real objectivity, and in the other there is a feature, considered by the investigator to be unique in the history of synæsthesia, viz: a phonism aroused by pain.

The two subjects, *M.*, a young woman student in the Cornell department of psychology, and *R.*, an undergraduate in the college, were subjected to similar series of experiments. The retinal colors were first carefully observed for 10 minutes and described. Then the photisms resulting from I. (*a*) simple noise and tone, (*b*) single tuning forks, (*c*) tuning-fork fusions, (*d*) piano compound clangs, (*e*) tones of church organ (various stops), (*f*) successive fork tones, (*g*) continuous tone change, (*h*) musical pieces on piano, phonograph and church organ; and II. (*a*) tastes (sweet, acid, salt and bitter), and (*b*) smell. Other synæsthetic phenomena such as colors and other characteristics of capital letters were observed. The photisms were projected, also, upon black, and colored, screens. No photisms could be produced by merely *thinking* the music. With both cases the relative position and shape of the colors were noted.

In the case of *M.* the violet end of the spectrum prevailed in most of the photism, while with *R.* red was a prominent color. The subjects projected the photisms upon a chart of colors arranged to show hue and saturation in slight gradations. The chart color that 'matched' in all cases was recorded and this method was used in all the experiments. With *M.*, the colors were frequently of highest saturation and luminous, the saturation being less with piano clangs than with fork tones and the clangs being less pleasant. Organ tone colors were clearer than those of piano clangs but paler than fork photisms.

In the case of *R*, the colors, while not always bright enough to project, were both definite and constant. The pitch and quality of mental auditions aroused by pain were for blunt pressures from a^1 to f^1 and for sharp pressures from b^1 to e^2 . One algometer pressure of 2.1 kg. produced c^4 .

It is to be hoped that the subjects will continue to be available for systematic research; and that there may be, later, enough experiments to warrant some generalization of results; which Mr. Whipple is at present careful to avoid.

WILFRID LAY.

EXPERIMENTAL.

Ueber die Eigenschaften der Schrift bei Gesunden. Von AUGUST DIEHL. Mit einer Figur im Text, Psychologische Arbeiten von EMIL KRAEPELIN, Dritter Band, 1 Heft, s. 1-61, 1899.

The aim of this research is, from an investigation of the writing of normal individuals of a given class of persons, to develop a basis for the interpretation of the writing of abnormal types of the same level of education. The specific problems guiding the investigation were inquiries, first, in general, as to the mutual relations which exist between the various factors involved in the writing process (extent of stroke, rate, pressure, time occupied in the actual inscription, time consumed by pauses), and, secondly, and more specifically, the effects of various conditioning influences such as practice, diversion, habit, variation in the emotional state, fatigue, etc., and especially to determine the results which followed from making the writing more difficult in various ways. To this latter end, the Arabic figures 1 to 10, were required to be written (1) carefully, the ordinary size, (2) as small as possible, (3) in the way easiest and most natural to the subject, (4) in the reverse order, from 10 to 1. The method followed was essentially the same as that of Gross in his 'Untersuchungen über die Schrift Gesunder und Geisteskranker' published in Vol. II. of the same journal. The chief improvement on the side of apparatus is his employment of the 'Curvenmesser' which makes possible a more accurate determination of the actual extent and rate of the writing-process.

Diehl's conclusions are briefly as follows:

1. The extent of the stroke decreases with the difficulty of the task imposed when this increase in the difficulty takes any of the following forms, an acceleration in the rate, an abatement of the stimu-

lus, or requiring the subject to write the subject-matter in the reverse order. On the other hand, the extent of stroke increases with the lightening of the task.

2. The rate slows up when the subject-matter is written in the reverse order, and is increased by a volitional impulse from within or by a stimulus from without.

3. The pressure increases when by reason of the increased difficulty of the task a volitional impulse is called out. It decreases with the ease of the task imposed.

4. The pauses in the writing are more influenced by the changes in the difficulty of the task than are the writing-periods.

5. The writing-periods for individual figures depend primarily on the extent of stroke, and therefore on the frequency and irregularity of the changes connected therewith. The position of the figures in the series is a determining influence.

6. Practice seems to reduce the size of the characters in writing without accelerating the speed; there is a decrease of pressure with practice also.

7. Under unfavorable emotional conditions there is a retardation of speed with an increase of pressure and a reduction in size of the characters.

8. Length of stroke, writing-time and writing-pressure are characteristics which vary together from one person to another. This does not hold for the rate.

9. The writing processes of different persons are comparable, thus, only when under exactly similar conditions.

10. Women write a larger hand and more rapidly and with less pressure, that is, more easily, than men, a conclusion exactly the opposite of that of Gross.

11. Men respond to an increased difficulty in the task imposed by an intensifying of the volitional impulse, the women by a reduction in the size of the characters written.

If this research has done nothing more than to show, as it does clearly, how fundamental the problems of rhythm and the complex inter-relationships of organic habits are to the study of the phenomena of such a process as writing, it would have been well worth the pains-taking work which has been put upon it. As the author says, this is only a beginning. But it is a beginning in the right direction and promises much.

H. HEATH BAWDEN.

NEUROLOGICAL.

Recherches sur la biologie de la cellule nerveuse. By G. MARINESCO. Archiv für Physiologie, 1899, Nos. 1 and 2. Pp. 89-111.

Observations, based on experiments, on the 'chromatophil' of the protoplasm of nerve cells, many valuable bibliographical references to the work of other neurologists, and considerations concerning the trophic relations of the neurones, form the chief substance of this paper by one of the numerous Italian investigators. A double-page plate made up of seven drawings of cell-structure adds something to the interest of the article.

To Professor Marinesco the nerve cell, whatever its morphology, seems to be composed in general of three essential elements: first, the chromatic substance called chromophil or chromatophil (from the ease with which it is stained); secondly, a 'figured' achromatic element (these two giving each cell its peculiar shape and character to the eye); thirdly, an amorphous substance, also achromatic, the fundamental protoplasm. The variable disposition and combinations of these three afford the neurones their very diverse appearance at different times.

The summary is of more interest to the psychologist perhaps than would be a detailed account of the often interesting observations and experiments. The part concerning the chromatophil is technically descriptive. Concerning cellular trophism so far as pertains to the nervous system, the author concludes as follows— notions which if substantiated would seem to bear not a little on the disputed continuity or discontinuity of the structural conducting units throughout the organism: "1. There is an intimate solidarity between the different constituent parts of the neurone. By interrelations, every portion plays a trophic rôle towards every other. When the protoplasmic prolongations or the prolongations of the axis cylinders are destroyed or injured in any way, the cell-body is the seat of constant reactionary changes which were unknown until within the last few years. Waller's theory therefore is obviously incomplete and inexact, inasmuch as it concentrates in the body of the cell all the trophic activities of the nerve. 2. There is a solidarity between the different neurones. Every disturbance in the function of a neurone affects the action of the adjacent neurone. For example, lesion of the sensory proneurone, following neural section or amputation, causes after a time trophic changes in the second neurone or in an indirect sensory neurone. To primary neural degen-

eration or atrophy there succeeds secondary nerve-atrophy. The process may go still further and attack the third neurone: there is then tertiary neuronal atrophy. The lesions of the cortical substance give rise to a like series of neural atrophies, but in an inverse way. 3. These facts cannot be understood if we admit, according to the classic theory, that the trophic influence is produced spontaneously in some way within the nerve-cell. For this notion of trophic automatism of the neurone, we substitute a theory which subordinates the neurone's life to the afferent (cellulipetal) and efferent (cellulifugal) excitations which pass from one neurone to the next. The functional and structural integrity of the neurone depends then at any time on the integrity of all its constituent parts and on that of the neurones which bring to it its functional stimulations. The neurone lives by its activity."

Observations on the Degeneration and Regeneration of Motor and Sensory Nerve Endings in Voluntary Muscles. G. CARL HUBER. *American Journal of Physiology*, Vol. III., No. VII., 1 Feb., 1900. Pp. 339-344.

In this not very novel research the posterior tibial nerve in each of fifty-seven rabbits was crushed, and observations made in each case afterwards to determine the progress of degeneration distal to the injury and later on in other rabbits as to the regenerative process. This nerve is peculiarly useful for this purpose because it connects with endorgans of three sorts in the muscles: neuro-muscular, neuro-tendinous, and sensory. Periods of from twenty-three hours to six days were allowed to elapse before microscopic examination was made in search of degenerative changes, and periods of from twenty-eight days to one hundred and seventy-eight days in the case of regeneration, the tissues being in both cases stained while alive, generally with methylene-blue. Functional differences and visible changes in the endorgans seemed to correspond regularly.

We may best perhaps quote the author's own "Conclusions: 1. The motor and sensory nerve endings of voluntary muscle degenerate after severance of the muscle nerves. The motor nerve endings and the extreme distal portion of the motor nerves degenerate earlier than do the sensory nerve endings. 2. The motor and sensory nerve endings in voluntary muscle may, under suitable conditions, regenerate completely. The motor nerve endings regenerate more quickly than the sensory nerve endings. 3. These experiments, it seems to me (although this is not emphasized in this report), show clearly that the regeneration of a degenerated portion of a peripheral nerve and its

termination is brought about by the down growth of the axis cylinder of the central, undegenerated portion of the nerve fibre. Further observations in this field are in progress."

GEORGE V. N. DEARBORN.

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COMPARATIVE PSYCHOLOGY.

Notes on the Psychic Development of the Young White Rat. WILLARD S. SMALL. American Journal of Psychology, Vol. XI., No. 1, Oct., 1899. Pp. 80-100.

An Experimental Study of the Mental Processes of the Rat. WILLARD S. SMALL. American Journal of Psychology, Vol. XI., No. 2, Jan., 1900. Pp. 133-165.

Notes on the Individual Psychophysiology of the Crayfish. G. V. N. DEARBORN. American Journal of Physiology, Vol. III., No. IX., 1900.

Studies on Reactions to Stimuli in Unicellular Organisms. VI. On the Reactions of Chilomonas to Organic Acids. H. S. JENNINGS. American Journal of Physiology, Vol. III., No. IX., 1900.

The first of Mr. Small's articles describes daily observations on white rats from birth to the age of twenty-eight days. Their responses to various physical stimuli were noted and conclusions drawn concerning the growth of their sense-powers, the presence of instincts and emotional reactions and of 'intelligence.' Such records are of great value in as much as an animal's sense-powers and unlearned responses are the basis of its mental capacities and growth. It is, however, a question whether the collection of miscellaneous observations cannot well be supplemented by more definite and rigorous, even if narrower, inquiries. When, for example, Mr. Small concludes that all tastes are 'unpleasant' to the newborn rat because it squeaks and wipes with forepaws at the sugar-solution, warm milk, salt solution and clear water applied to its lips, one wishes that he had made more definite tests. It may not have been the taste at all to which the animal reacted.

Mr. Small's comments on his observations retain something of the vague phraseology and anthropomorphic descriptions of the popular writers on 'intelligence.' Surely one can find more scientific descriptive language than the following: 'One thus insulted,' 'Saw one seeking purposively,' 'declined to eat sealing-wax,' 'advancing intelligence

was marked to-day by a rat's pushing a comrade away from the dish of milk,' 'as if affectionately fondling each other,' 'they are veritably lieblichste Thierchen,' 'I think there is a considerable fund of altruism in the rat nature.'

Mr. Small tested the mental processes of adult and young rats by observing their methods of learning to overcome some simple obstacle which withheld them from food. Digging at a certain place and removing strips of paper which held a door, were the acts concerned. The field of experimentation was thus small, but the animals' behavior is recorded in minute detail and a rather elaborate analysis of the process is attempted. No signs of inference or of imitation in the sense of learning to do a thing from witnessing another do it, were seen. Mr. Small writes as if he thought that the animals concerned had habitually 'ideas' of being in and getting into the box, of eating, etc., but he offers no evidence to refute the evidence on the opposite side which I presented in 'Animal Intelligence; an Experimental Study.'

The patience shown by Mr. Small in making such detailed records is admirable, though it must be confessed that it takes some patience to read them. This article like the previous one suffers from a number of anthropomorphisms which I take it are due to carelessness of rhetoric rather than to any real misconception of animal mentality.

Dr. Dearborn describes a large number of observations on the sense-powers and behavior of the crayfish. His aim was to discover the range of variation in a number of functions among individuals and in the same individual at different times. Constancy in the performances of an individual was taken to be evidence of 'individuality.'

The range of variation in both respects was found to be very large, and no definite correlations (*e. g.*, between slowness in one test and slowness in another) could be made out. It is possible that this variation in results may have been due in part to variations in the stimulus or situation to which the animal responded. It is a very delicate task to keep the conditions of a psychological experiment constant when your subject is an animal which is sensitive to stimuli which human beings in the same situation would not notice. In experiments on traction strength, for instance, "the animal was set upon the wet towel and gently tapped with a glass rod." Here the variation in the force exerted by a crayfish at different times may have been partly due to variations in the force and place of the tap. Again we do not know that the animal in every case stopped walking ahead because it could pull the spring no farther.

It should be noted that there is a good deal of constancy in these

animals' behavior, after all; *e. g.*, when tapped, they *invariably* walked forward, though they varied exceedingly in the force they executed. The most important case of variation found by Dr. Dearborn concerns the simple ability of a crayfish to right itself when put on its back on a smooth plane surface in water. Some did so easily; some with difficulty; some never could.

In a brief paper Dr. Jennings seeks to explain the phenomena of aggregation which Garrey demonstrated in the case of *Chilomonas*, as results of the motor reflex described in Jennings' earlier papers. It is unfortunate that Garrey has not made use of Jennings' method and so gives no opportunity for an exact comparison of observations. If Jennings' account of the motor reaction of *Chilomonas* is correct, it certainly seems sufficient to explain the phenomena noted by Garrey. It is to be hoped that the significance of Jennings' studies will bring about their repetition by other observers.

EDWARD THORNDIKE.

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NEW BOOKS.

Völkerpsychologie; eine Untersuchung der Entwicklungsgesetze von Sprache, Mythos und Sitte. WILHELM WUNDT. I. *Die Sprache*, Leipzig, Engelmann. 1900.

Das soziale und sittliche Leben erklärt durch die seelische Entwicklung. J. MARK BALDWIN. Translated from the second English edition by DR. R. RUEDEMANN, with preface by DR. PAUL BARTH. Leipzig, J. A. Barth, 1900. Pp. xv + 466.

Annales de l'Institut international de sociologie, VI. R. Worms. 1900.

Introduction a la vie de l'esprit. L. BRUNSWIEG. Paris, Alcan. 1900. Pp. 176.

Medicine and the Mind. M. DE FLEURY. Trans. by S. B. COLLINS. London, Downey & Co.; New York, imported by Scribners. 1900. Pp. xii + 373.

Outlines of the Comparative Physiology and Morphology of Animals. JOSEPH LECONTE. New York, D. Appleton & Co. 1900. Pp. xviii + 499.

Dreams of a Spirit-Seer, illustrated by Dreams of Metaphysics.

IMMANUEL KANT. Translated by EMANUEL F. GOERWITZ, edited with an introduction and notes by FRANK SEWELL. London, Swan, Sonnenschein & Co., Lim.; New York, The Macmillan Company. 1900. Pp. xiv + 161.

Immanuel Kant's Kritik der reinen Vernunft. B. ERDMANN. Berlin, Reimer. 1900. (5th edition.) Pp. 609.

The White Robe of Churches of the XIth Century. H. D. M. SPENCE. London, J. M. Dent & Co., imported by Charles Scribner's Sons. 1900. Pp. xx + 348. \$3.

NOTES.

IT is proposed to celebrate the 70th birthday of Professor Wilhelm Wundt, which will occur on the 16th of August, 1902, by the publication of a *Festschrift* to which his former students are invited to contribute. The manuscripts must be forwarded to Professor Külpe, Würzburg, not later than January 1, 1902.

ABERDEEN UNIVERSITY has conferred the degree of Doctor of Laws on Professor Josiah Royce, of Harvard, who recently completed the second series of Gifford Lectures before the University.

PROFESSOR J. MARK BALDWIN has been given the degree of D.Sc., by Oxford University, this being the first occasion on which this degree has been conferred by Oxford.

PROFESSOR E. B. DELABARRE is this summer conducting an expedition to Labrador.

THE death is announced at the age of 87 years of M. Ravaisson Mollien, formerly professor of philosophy at Rennes, inspector general in the Department of Higher Education and curator in the Department of Antiquities at the Louvre. He was the author of many works on philosophy and æsthetics. The death is also announced of M. Hippolyte Stupuy, at the age of seventy years. He was curator of the artistic collections of the City of Paris, and the author of works on philosophy and of a biography of the mathematician, Sophie Germain.

By the will of the late Jonas G. Clark, Worcester, Mass., who founded Clark University in 1889, the entire estate is left to the uni-

versity, providing the people of Worcester raise a fund of \$500,000. If the sum of \$250,000 is raised, he bequeaths \$500,000. If \$500,000 is raised, he bequeaths \$1,000,000 and makes the university his residuary legatee. He also leaves \$100,000 for the university library and \$100,000 for a department of art.

THE announcement of the Department of Philosophy of Harvard University contains some additional courses and changes. Dr. R. B. Perry has been appointed Austin teaching fellow, and will give the course on the philosophy of Kant which has been given by Professor Palmer. Dr. Miller will give the second half of the introductory course in philosophy in the place of Professor Royce. Professor Santayana offers a course in Aristotle's *Metaphysics*.

THE following promotions have been made in the Philosophical Department of the University of Michigan: George Rebec, Ph.D. (Michigan), instructor in philosophy, to be assistant professor of philosophy; W. B. Pillsbury, Ph.D. (Cornell), instructor in psychology, to be assistant professor of psychology and director of the psychological laboratory.

DR. ARTHUR H. PIERCE, of Amherst College, has been appointed associate professor of mental and moral science at Smith College.

DR. MAX MEYER, formerly assistant in the psychological laboratory at Berlin and this year honorary fellow in psychology in Clark University, has been appointed professor of psychology in the University of Missouri.

HERBERT G. LORD, A.M. (Amherst), has been appointed professor of philosophy in Columbia University. He will have charge of the introductory collegiate courses.

DR. C. M. BAKEWELL, of Bryn Mawr College, has been called to a professorship of philosophy in the University of California and will be succeeded at Bryn Mawr by Dr. David Irons, of Cornell University, who will have the title of 'Associate.'

MISS MARGARET F. WASHBURN, Ph.D., professor of philosophy in Wells College, has been appointed Warden of Sage College, Cornell University.

MR. J. FRANK MESSENGER, B.A. (Kansas, 1895), has been appointed assistant in the psychological laboratory, Harvard University.

DR. ARTHUR WRESCHNER has qualified as docent for philosophy and psychology at Zurich. The subject of his inaugural address was 'The Influence of Leibnitz on pre-Kantian Psychology and Aesthetics.'

